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ESTABLISHMENT OF THE NERVOUS SYSTEM

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INTRODUCTION

IN THE following paper I should like to apply the most recent method used in modern zoology, that of experimentation, to the ancient problem of substance and pattern. Our question is: How is the substance of a single cell, the ovum, transformed into a pattern of many diverse systems, including the complex nervous system? To the early biologist who regarded the structures characteristic of the adult as preformed in the egg, development of a complicated pattern meant only the growth of the preformed rudiments into the adult systems.

Roux (1883), who early studied the causal relations underlying development, postulated cleavage of the egg as a factor in its unfolding. He assumed that the two daughter nuclei, resulting from the division of the original fertilization or fusion nucleus, determine the first two cells or blastomeres for right and left sides of the body, and that succeeding early daughter nuclei set the succeeding blastomeres for particular parts, thus building up a mosaic like a tile pattern. Hertwig (1893) showed, however, that through pressure on the dividing egg the early daughter nuclei may be shifted into blastomeres which are foreign to them, and that these blastomeres regardless of their strange nuclei give rise to their proper parts of the embryo. Evidence of this type indicated that in early cleavage the nuclei do not set the cytoplasm for particular ends.

Spemann (1928) appears to have proved this point. By placing a noose around the jelly of an egg of the European newt, *Triton taeniatus*, and constricting the egg in such a way that both the male and the female pronuclei (*n.*, fig. 1a) remain on same side of the noose, he showed that the side containing the fusion nucleus (fig. 1b) develops, but that the cytoplasm on the opposite side of the noose does not develop *unless* a daughter nucleus enters it. If, however, a one-eighth or even a one-sixteenth of the original fusion nucleus passes over the uniting bridge into this cytoplasm, then this cytoplasm also is activated to development. But what it develops into depends not upon the daughter nucleus entering it, as Roux had postulated, but upon the cytoplasm itself. Thus, if we assume that in figure 1b the

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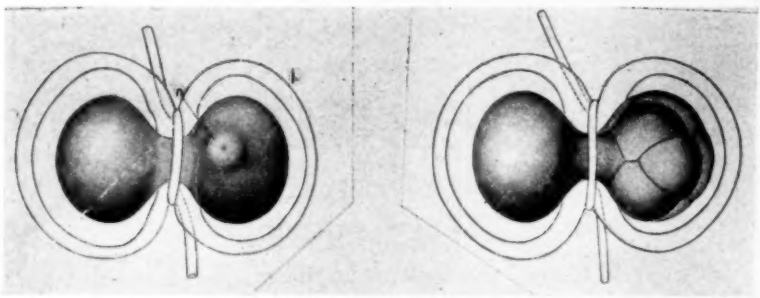
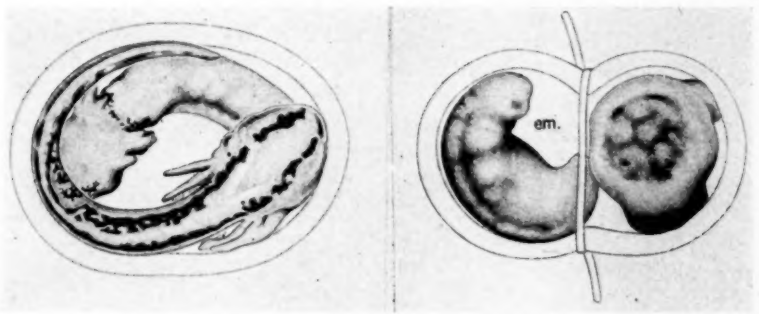
noose is so tied as to bisect the so-called gray crescent (dorsal organs), then the cytoplasm in which the original nucleus is located will form a *complete* embryo and not a half, and the cytoplasm on the opposite side of the noose into which the daughter nucleus enters late will also form a whole embryo; so that by this sagittal constriction twins result (fig. 2a). If, on the contrary, the constriction is frontal, separating the dorsal organs from the yolk, then the cytoplasm of the dorsal organs will produce an embryo (*em.*, fig. 2b); while the other half into which one-eighth nucleus enters may form parts of all of the other germ layers, yet it will not produce an embryo. This experiment demonstrates that while one-eighth of the original nucleus is sufficient to instigate development, it is the cytoplasmic substance itself which is responsible for the pattern.



a *b*

FIG. 1. CONSTRICTION OF THE EGG OF TRITON TAENIATUS

a. A noose is placed around the jelly (*j.*) in such a way that both male and female pronuclei (*n.*) remain on the same side of the noose. *b*. The cytoplasm on the side containing the fusion nucleus has undergone cleavage, while that on the opposite side (without a nucleus) has not developed. (After Spemann, 1918.)



a *b*

FIG. 2. DEPENDENCE OF THE PATTERN OF DEVELOPMENT UPON THE CYTOPLASM

a. Twins of *Triton taeniatus* resulting from sagittal constriction of a single egg, bisecting the gray crescent (dorsal organs), a daughter nucleus having entered the non-nucleated cytoplasm late from the opposite, developing side. *b*. Normal embryo (*em.*) and mass of cells which result if the noose is so placed as to form a frontal constriction separating the dorsal organs from the yolk. (After Spemann, 1918.)

A. DETERMINATION OF THE MEDULLARY PLATE

(*a*) *In normal development*

From the above experiment of Spemann it appears that the region early occupied by the gray crescent (dorsal organs) is a center of organization from and around which important systems of the embryo are built. Vogt (1929), by means of vital dyes, has mapped out this area as an oblique cresent (chorda; somites; fig. 3) around the egg and has shown its surface

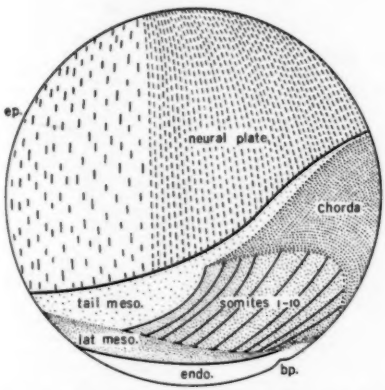


FIG. 3. SURFACE RELATIONS OF THE REGIONS OF THE YOUNG GASTRULA PREVIOUS TO THE FORMATION OF THE MEDULLARY PLATE
(After Vogt, 1926.)

relations to other regions previous to the formation of the neural plate; and Marx (1925) has demonstrated that when this center of organization, as the roof of the early digestive tract, comes to underlie and to press up against the surface layer of ectoderm (neural plate, fig. 3), it transforms the ectoderm into the rudiment of the central nervous system. Further, I have recently shown (Daniel, 1936) that this region may be followed in the living egg as it rolls in to form the roof of the digestive tract, and that certain details of its relation to, and contact with, the ectoderm as the ectoderm is tansformed into the medullary plate may be made out.

If we look at a transverse section through the region after the ectoderm has been changed into the medullary plate (*mp.*, fig. 4), we see that the plate lies in close contact with the original chordamesoderm (*ch.* and *so.*) which at this stage has already lifted up and so no longer forms the roof of the digestive tract (*dg.*). In such a section it is observed that where

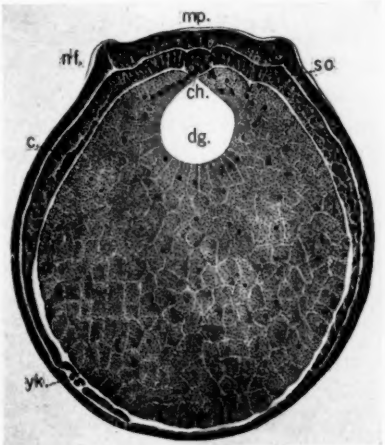


FIG. 4. TRANSVERSE SECTION OF NEURULA OF TRITURUS TOROSUS

Medullary plate region (*mp.*) which thins out over the notochord (*ch.*) as a median groove. Where the outer part of the somite (*so.*) touches the medullary plate the plate cells elongate and form the neural folds (*n.f.*).

the notochord (*ch.*) comes into contact with the medullary plate, the plate early thins out above as a median longitudinal groove, and protrudes ventrally, just above the notochord into a longitudinal neural keel (Baker, 1927). But equally as important is the observation that where the outer part of the somite (*so.*) touches the medullary plate, the plate cells elongate and rise up as neural folds (*n.f.*), roughing out the medullary plate into its definitive spatulate pattern (fig. 6a).

(b) In an accessory embryo

Just a little over a decade ago Spemann and Hilde Mangold (1924) performed the significant experiment which first demonstrated the influence of the center on the organization of an embryo. They showed that if a piece is taken from the organizing center in *Triton* and placed under indifferent ectoderm of another embryo, the ectoderm will be induced to form a nervous system; further, the piece will organize the subjacent host tissue, completing an accessory or secondary embryo or a piece of the brain of a tadpole, may also act as an organizer.

(c) In a foreign position

One of my students, Richard M. Eakin (1933), has shown that if the blastocoele, or segmentation cavity of the egg, be filled with a substance like gelatin which will solidify when cool, then the organizing center is unable to roll into the egg to form the roof of the digestive tract. Under this experimental condition this center (chorda; somites; fig. 3) is forced

FIG. 5. A SECONDARY EMBRYO HAS BEEN INDUCED IN AN EMBRYO OF TRITURUS TOROSUS

Induced by a piece of organizing center under indifferent ectoderm; *nt.* and *nt.*², neural tubes of normal and induced embryos, respectively. *a.* External view from the right side. *b.* Cross section of the double embryo; *ar.*, archenteron; *ch.*², implanted chorda; *so.*², implanted somite. (After Schechtman, 1934.)

(figs. 5a-b). Moreover, a piece put into the blastocoele and brought by gastrulation into contact with the overlying ectoderm will similarly induce a secondary embryo (fig. 5a at *nt*²). Bautzmann (1926) showed that the organizing center includes the dorsal and lateral lips of the blastopore (chorda; somites; fig. 3), that is, the oblique crescent of Vogt. He further (1928) showed that a piece of the young notochord may act as an organizer. O. Mangold (1929) added that a piece of the medullary plate after being induced, to occupy an unusual position. Fixed at its place of attachment to the entoderm, the organizing center folds down over the yolk area with one layer of the fold deeper and the other superficial. This migration may continue until most (or all) of the organizing area has rolled under as the deeper layer, so that what should be the roof of the digestive tract now extends downward across the yolk. The unique thing in this experiment is that the organizing center, even in this foreign environment, is still capable of inducing the over-

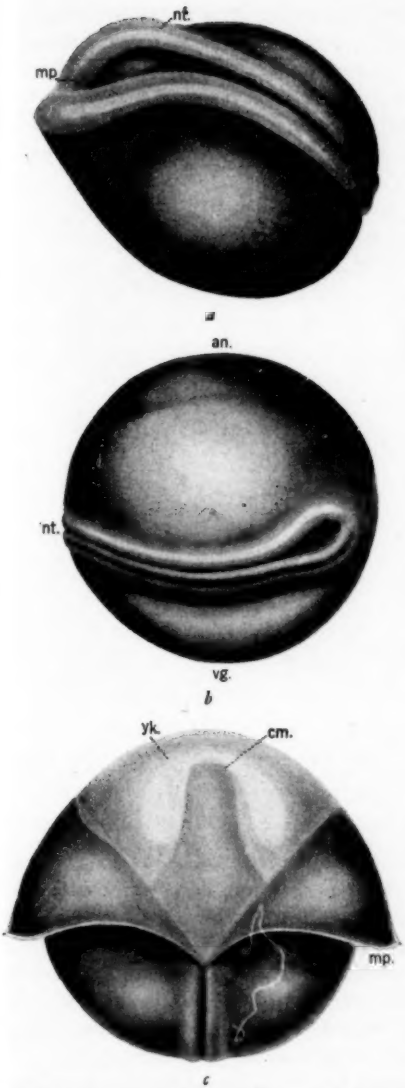


FIG. 6. INDUCTION OF MEDULLARY PLATE ON VENTRAL SURFACE OF EMBRYO

a. Normal neurula of *Triturus torosus*; *mp.*, medullary plate; *nf.*, neural fold. *b*. The blastocoele has been filled with gelatin, preventing the invagination of the organizing center, which then folds down over the yolk area and induces a medullary plate on the ventral surface. *c*. The medullary plate (*mp.*) has been cut and thrown back to show the underlying organizing center (*cm.*) lying against the yolk (*yk.*). (After Eakin, 1933.)

lying (underlying) layer to form medullary plate (fig. 6b), although this localizes the medullary plate on the belly instead of in its normal position on the back.

In figure 6c an incision has been made longitudinally through the neural tube, and the sides of the surface layer (*mp.*) have been thrown back so as to show the deeper layer as the organizing center of Spemann (*cm.*) lying against the yolk (*yk.*).

From the above experimental studies it is seen that the rôle of the organizing center is the same in these three experiments. Wherever this center comes into contact with ectoderm, there the simple ectoderm takes the outline of the medullary plate which later develops as the central nervous system.

But does this association of the center of organization and ectoderm demonstrate that the medullary plate owes its determination wholly to the center of organization?

Separation of the center of organization and the tissue to be induced

In recent experiments Holtfreter (1933a, b) has shown that ectoderm which should normally go to form medullary plate, if isolated from the organizing center, is not so transformed. By allowing the denuded eggs of Axolotl to develop in a weak Ringer's solution instead of in water, the yolk and chordamesoderm, instead of invaginating, may evaginate so as to produce of the egg a dumbbell-shaped figure (fig. 7a), one part of the dumbbell being produced by the evaginated yolk and chordamesoderm (*cm.*) and the other part by the ectoderm (*ec.*). In this experiment an almost complete separation of the ectoderm (*ec.*) from the organizing center (chordamesoderm [*cm.*] and entoderm, figs. 7b-c) is effected. Without the determining influence of the chordamesoderm, however, the ectoderm (*ec.*, fig. 7c)

shows no signs of differentiating into medullary cells. This experiment indicates that the presumptive medullary material is dependent for its determination as a medullary plate upon contact with the organizing center.

ning of gastrulation as a crescent (stippled in fig. 8a) near the animal pole, and that it then moves into its definitive position to form the spatulate medullary plate (fig. 8b). Goerttler further made transplants of this presumptive medullary crescent

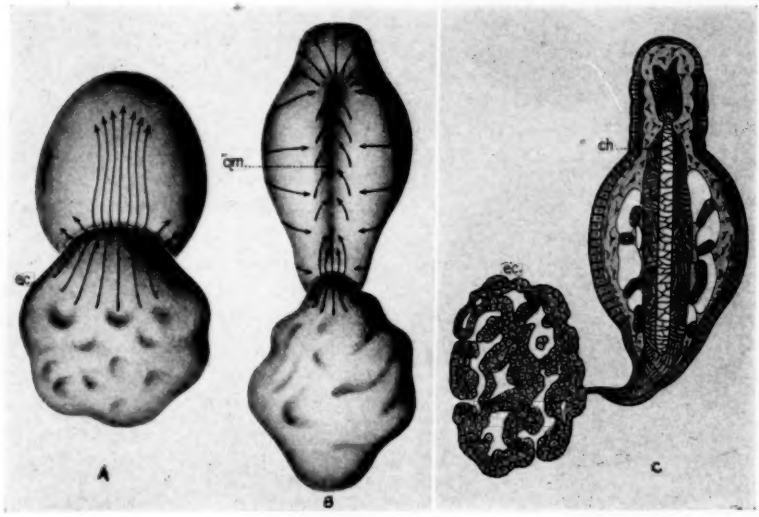


FIG. 7. EXOGASTRULA

Denuded eggs of Axolotl have been allowed to develop in weak Ringer's solution, which causes the yolk and chordamesoderm (*cm.*) to evaginate, thus bringing about almost complete separation of the ectoderm (*ec.*, fig. 7c) from the organizing center. Without the influence of the chordamesoderm the ectoderm does not differentiate into medullary cells. (After Holtfreter, 1933a.)

Is determination of medullary plate possible in the absence of an organizing center?

(a) Experimental separation of presumptive medullary material from the organizing center

There have been data amassed indicating that the medullary plate may be partly determined before it comes into contact with the organizing center. If this be true, the organizer would act only in reinforcing induction. Goerttler (1927), by the vital staining method, has shown that the substance which goes to form the medullary plate is localized at the begin-

before it had moved to its final position and hence before it had been underbedded by the roof of the digestive tract, in order to test whether at that time it was partly determined. A piece (*ex.*, fig. 8a) transplanted to an older neurula (fig. 8b), and so oriented as not to disturb its own directional movements (shown by arrows), resulted in a small accessory medullary plate (*mp.*², fig. 9). This experiment Goerttler held to demonstrate early (labile) determination of the medullary material independent of the center of organization. Holtfreter (1933c) has criticized this interpretation, however, maintaining that the pieces so transplanted were in

contact with mesoderm or with nerve tube, both of which are known to have organizing capacities.

(b) Isolation and cultivation of presumptive medullary material (*in vitro*)

If material presumed to be neural or medullary (see fig. 3) be determined or partly determined at this early period, its removal and cultivation outside the egg might be used to test this capacity.

Erdmann (1931), employing this method on *Amblystoma*, concluded that the presumptive medullary material grown outside the egg in a weak Ringer's solution developed as medullary material. Schechtman (1934), in our laboratory, has studied this problem at length on *Triturus torosus*. He has found that pieces removed from the presumptive medullary plate and cultured in Holtfreter's solution before they have been underbedded by the roof of the digestive tract may indeed possess elongate cells (fig. 10a) resembling medullary cells. A careful examination of these cells, however, in a more recent study (Schechtman, 1935) has brought forth several points of special interest. He has observed that in an explanted piece curling usually occurs, and that the curling tends to make some of the cells wedge-shaped. He has observed, further, that the explant also contracts, rendering the cells still more slender and elongate, and hence still more like medullary cells (fig. 10b). It is readily shown that both of these tendencies of the explant to curl up and to contract, mechanically influence the form of the cells; and that both are due to the operational procedure.

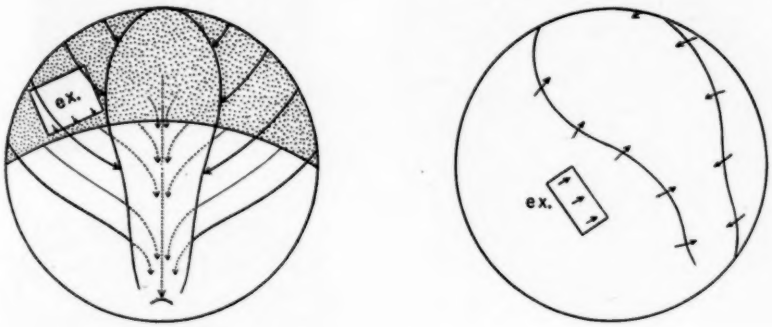


FIG. 8. PURPORTED EARLY DETERMINATION OF MEDULLARY MATERIAL INDEPENDENT OF ORGANIZING CENTER
The stippled area in *a* shows the material which will form the medullary plate, localized as a crescent near the animal pole at the beginning of gastrulation. A piece of this crescent (*ex.*) was transplanted to an older neurula (*b*), where it formed a small accessory medullary plate (see fig. 9). (After Goerttler, 1927.)

If, however, these explants now be cultured for a short period in Holtfreter's solution, their cells become cuboidal or low columnar in shape, only occasional elongate cells remaining. If these cultured explants be compared with cultured explants which have been underbedded by chordamesoderm and hence certainly determined (fig. 10c), the difference between the two is striking. The true medullary cells occur as a compact mass, while there is paucity of elongate cells in the area presumed to be medullary.

Another indication that they are non-

medullary may be found in the fact that similar elongate cells also occur in explants of the presumptive epidermis which we know are not determined or even partly determined as medullary material.

We may answer our question in part by repeating that wherever an organizing center like the chordamesoderm comes into contact with ectoderm, there medullary structures are produced. In the absence of such contact, on the contrary, we must conclude that determination or even partial determination has not as yet been demonstrated.

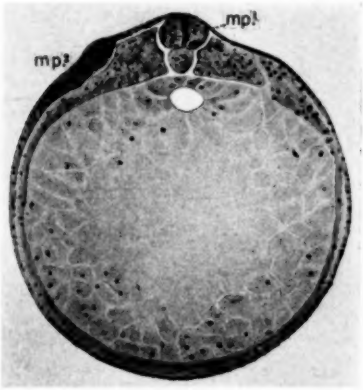


FIG. 9. ACCESSORY MEDULLARY PLATE (*mp.*²) FORMED BY A TRANSPLANT OF PRESUMPTIVE MEDULLARY PLATE MATERIAL FROM AN EARLY GASTRULA TO AN OLDER NEURULA
(After Goerttler, 1927.)

Induction by killed material

Above we have considered the organizing center of Spemann as a source of the inductor which transforms ectodermal material into the medullary pattern, but we have also seen that pieces of young notochord, induced medullary plate, and young brain may also be used as inductors. A paper by Bautzmann, Holtfreter, Spemann, and Mangold (1932) has added the important findings that medullary structures may be induced by materials that have been killed by drying, boiling, or freezing, as well as by the living tissues. Indeed, pieces from gastrulae or neurulae, preserved in formalin

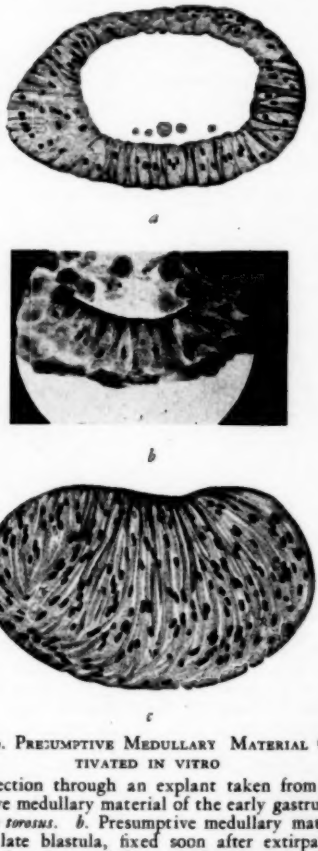


FIG. 10. PRESUMPTIVE MEDULLARY MATERIAL CULTIVATED IN VITRO

a. Section through an explant taken from presumptive medullary material of the early gastrula of *Triturus torosus*. *b*. Presumptive medullary material from a late blastula, fixed soon after extirpation. The explant has curled outward. Note the long, slender cells. *c*. Section through an explant taken from medullary material of the late gastrula of *Triturus*. (After Schechtman; a and c, 1934; b, 1935.)

for six months, are still effective medullary inductors; and pieces kept in 20 per cent hydrochloric acid for some time and then washed will still induce medullary structures from presumptive ectoderm. Pieces, however, which have been put

into boiling paraffin or which have been kept an hour and a half in ether, lose wholly or in part their ability to induce (Holtfreter, 1933 d, e).

In these experiments it has also been shown that certain tissues which are impotent as inductors in the living state are potent if killed. Thus, if a piece of presumptive epidermis which in the living condition is unable to induce, be killed and placed between two layers of presumptive epidermis, then this dead presumptive epidermis will induce medullary structures from the layers of presumptive epidermis. Or if a small bit of presumptive epidermis be placed on a larger sheet of killed presumptive epidermis, it will develop into medullary structures. Moreover, entoderm or yolk material, if killed, may be similarly used as an inducing agent for presumptive epidermis (fig. 11). Or even pieces of the fertilized egg or zygote, if killed, may be used effectively as inductors.

Holtfreter (1933e) has further shown that the ability to induce is present in tissues of some invertebrates and all vertebrate classes. The tissues of the liver, heart, spleen, muscles, or ovarian eggs of fishes, when put under the presumptive skin of *Triton*, induce large neural structures; and pieces of the liver, kidney, testes of the common lizard, *lacerta agilis*, give brain-like structures. The liver, kidney, testes, and thyroid of birds are capable of inducing large medullary plates. Furthermore, an extract of chick embryos cultivated outside the egg and heated to form a coagulum produces neural structures above this coagulum as it would above a piece of an organizer. Also, other structures of a secondary embryo may be produced in *Triton*, as, for example, notochord, muscles, and kidney tubules. In a mammal like the mouse, the liver, kidney (fig. 12), heart, brain, orlens of the adult will produce from the material of the side of the embryo of the newt *Triton* an accessory embryo with large neural structures, a notochord, muscles, kidney, and connective tissue. Even pieces of the human liver, brain, thyroid, or certain muscles will induce neural structures, notochord, and muscles of an accessory embryo of *Triton* as would the organizing center of Spemann.

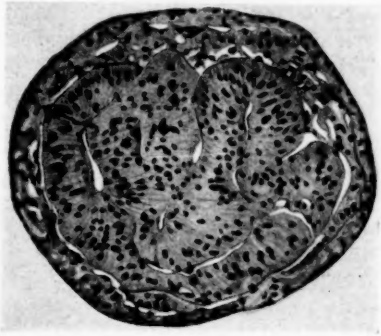


FIG. 11. MEDULLARY STRUCTURES INDUCED IN PRESUMPTIVE EPIDERMIS BY KILLED ENTODERM OR YOLK MATERIAL
(After Holtfreter, 1933d.)

Presence and nature of the medullary inductor

It would appear from a summary of all these studies that the stuff, whatever it be, which determines the medullary plate is either particularly abundant in the organizing center of Spemann, or is available to a great degree in this center. In certain living embryonic layers, as, for example, ectoderm or entoderm, it is not available. If these be killed, however, by any of the ways indicated above, then they too are capable of inducing medullary structures in presumptive epidermis. In these, either the inducing substance is not present in the living tissues and is a product of the killing process, or it is present but not available until the tissue is killed. It has further been demon-

strated that it (or something comparable to it) is present in different tissues of a great group of animals, and is capable of inducing not only medullary structures but also of organizing the host tissue of *Triton* into an accessory embryo; and that the accessory embryo resulting is practically as complete as is an accessory embryo produced by the organizer of Spemann.

Recent work has sought to ascertain the nature of this inductor. The fact that induction can be brought about by killed material indicates strongly that the inductor is chemical in nature. Fisher and Wehmeier (1933) have argued that it appears to be animal starch or glycogen. But Waddington and Needhams (1933) have shown that an extract from young chicks is inducing, although chicks at seven days have practically no glycogen present. Recently Barth (1934) has succeeded in producing neural plates in neurulae of Axolotl by means of cephalin. Whether induction results from the cephalin itself or from impurities in the cephalin is yet to be ascertained.

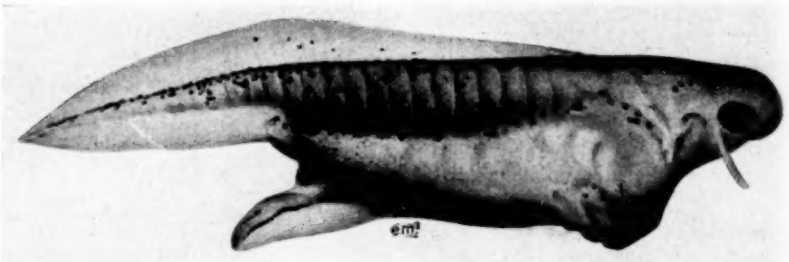


FIG. 12. INDUCTION OF AN ACCESSORY EMBRYO (*em.*²) ON THE SIDE OF AN EMBRYO OF TRITON BY TISSUE FROM THE KIDNEY OF THE MOUSE
(After Holtfreter, 1933e.)

B. DEVELOPMENT OF THE NEURAL TUBE

Its general regions and closure

The normal medullary plate early roughed out from the ectoderm becomes the brain and cord. That the general regions for the brain and cord owe their determination to the underlying chordamesoderm is shown by the fact that if three similar pieces of gastrular ectoderm be placed over the forward, middle, and last thirds of Holtfreter's exogastrula (fig. 7b) *before* the entoderm flows over the chordamesoderm, the anterior piece will produce brain together with other structures of nose, eye, and ear pertaining to this segment, and the middle and posterior pieces will form the longitudinal spinal cord.

As the medullary plate begins to sink in the midline along the median longitudinal groove and to fold up along its periphery by the elevation of neural folds, the neural tube as such begins to take form. The sinking in the midline extends forward above the median longitudinal groove and keel to just behind what will be the infundibular recess. The folds, as we have seen, are produced by the elongation of cells directly above the outer margins of the somites (*so.*, fig. 4). The more lateral of these cells which finally become the neural crests (Baker, *loc. cit.*; Landacre, 1921), rise rapidly and fuse first just behind the ear capsules (fig. 13 b). From this point they close slowly backward over the spinal cord,

and a bit more promptly forward over the brain area (*Triturus*).

FIG. 13*a–c*. GASTRULA (A) OF TRITURUS TOROSUS

Gastrula stained with vital dyes (1–12) to show extension of areas; in neurula (*b*) stains 1–6 show neural folds elongating; in larva (*c*), *md.*, mandible; neural fold closed, hiding stains 1–6. (After Schechtman, 1932.)

During and following the closure of the tube Schechtman (1932) has shown that there is great stretching in a longitudinal direction in *Triturus torosus*. Vital dyes placed on or near the horizon of the gastrula (*1–12*, fig. 13a) shift their positions in the neurula, stains 7–12 bending with the flexure of the neural folds and stains 1–6 stretching longitudinally in the cord of the larva (fig. 13b). In figure 13c the stains (*1–6*) have been enclosed within the elongated spinal cord.

In addition to marking out the main regions of the neural tube, the roof of the digestive tract controls the details of the pattern, as has been shown in *H. regilla* by another of my students, A. L. Alderman (1935). As the anterior part of the brain, the prosencephalon, depends on its substratum for its pattern, so does the eye, a derivative of that part of the brain, depend on a specific substratum. Thus, if that part of the forebrain which should give rise to the eyes be underbedded by a part of the roof just back of the eye and transplanted to the ventral region, the piece of prosencephalon will now form brain and not eye.

Differentiation of motor and sensory neurones and the establishment of descending and ascending tracts

(a) Motor neurones

The differentiation of the cells within the neural tube of *Amblystoma* has been studied by Baker (1927) and by Coghill (1929). After the medullary plate has been determined, the cells just under and at the sides of the median longitudinal groove (figs. 4 and 14) form the neural keel. The keel extends farther ventrally and comes into so close physical relation with the notochord and mesoderm that these structures cannot be separated from it without injury (Coghill). Later the neuroepithelial cells in this region of the cord construct from their outer ends the external limiting membrane, separating the keel from the mass of mesoderm. The

cells in the keel itself now orient themselves radially with their outer, broader ends toward the mesoderm and notochord, and their narrower pigmented ends directed inwards. The pigmented ends of the cells on one side of the keel now pull away from their fellows on the opposite side, leaving a cleft which further deepens the median longitudinal groove.

Neuroepithelial cells in the keel now differentiate into the first localized center of primary motor cells (*m.c.*, fig. 14a).

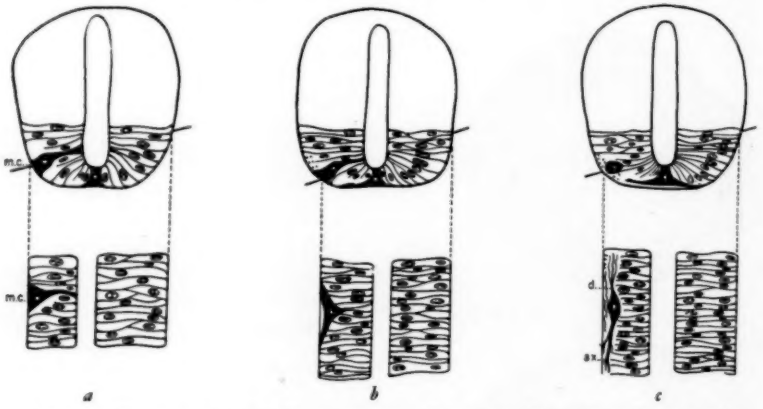


FIG. 14. DIFFERENTIATION OF NEUROEPITHELIAL CELLS INTO NERVE CELLS (NEURONES)

The upper figures represent sections transverse to the longitudinal axis of the spinal cord; the lower figures, sections parallel to the longitudinal axis and in planes indicated by the lines to which the dotted lines lead from the lower figures. Concentration of the cytoplasm of the differentiating cells (*m.c.*) against the external limiting membrane, the drawing away of the central part of each cell from the membrane, and the elongation of the tips (*d.*, dendrite; *ax.*, axone) are shown successively in *a*–*c*. (After Coghill, 1929.)

The manner in which this differentiation takes place, according to Coghill, is as follows: The protoplasm and nuclei of ventral neuroepithelial cells move toward the external limiting membrane. The central part of the body of the cell draws away from the membrane, leaving only the anterior and posterior extremities attached. The extremities of the cell next to the membrane now become the growing tips, thus giving a longitudinal neurone (*m.c.*, fig. 14a).

Both growing tips creep along the external limiting membrane but in opposite directions. One becomes the axone (*ax.* fig. 14c); the other, the dendrite (*d.*). The motor axones (*ax.*, fig. 15) pass *down* along the limiting membrane of the tube, forming the descending motor tract. The direction of their growth, according to Coghill, is in response to the gradient within the unsegmented mass of mesoderm (*m.*, fig. 16). Motor axones grow against this current and consequently down the external limiting membrane of the tube. Motor dendrites (*d.*, fig. 14c) go with the current and hence up the tube.

(b) Sensory neurones

Primary sensory cells are anatomically like primary motor cells in that they lie within the walls of the neural tube (spinal cord) and form longitudinal tracts, but the sensory cells arise from the neuroepithelial cells in the dorsolateral walls of the tube. A single growing tip of the cell faces the external limiting membrane

and then bifurcates, its two parts creeping in opposite directions along the membrane. The dendrite (*d.*[1], fig. 15) runs backward with the current and the axone proper (*ax.*[1]) runs forward against the

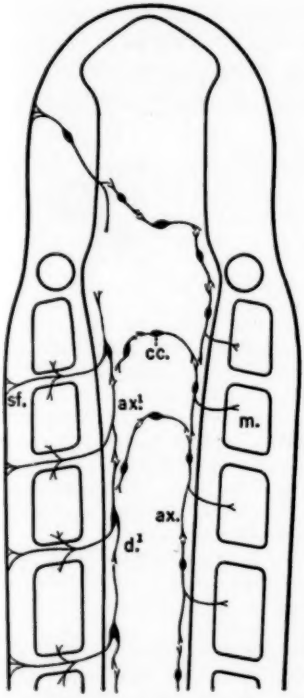


FIG. 15. DIAGRAM OF THE RELATION OF THE DEVELOPING NEURON OF AMBLYSTOMA TO THE MUSCLES AND SKIN (SEEN IN FRONTAL SECTION)

The descending motor axones (*ax.*) making up the motor tract are shown on the right; the ascending, sensory axones (ax[1]) making up the sensory path are on the left; and the commissural (floor-plate) cells (*c.*) in the center. (After Coghill, 1929.)

ectodermal current, and with other similar axones forms the ascending tract.

Differentiation of commissural neurones and the union of ascending and descending tracts

Some of the neuroepithelial cells which lie in the floor plate directly under the neurocoele (*cc.*, figs. 16 and 15) form growing tips which creep laterally right or left toward the mesoderm and then turn posteriorly in right or left motor paths. A second growing tip then forms from such a cell and, as a dendrite, grows laterally in the direction opposite the originating axone. The dendrites of this commissural or floor-plate cell (*cc.*) are then put into communication with the sensory path of the opposite side by a second

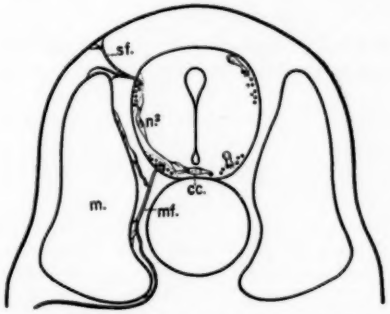


FIG. 16. DIAGRAM OF THE RELATION OF THE DEVELOPING NEURONES OF AMBLYSTOMA TO THE MUSCLES AND SKIN IN THE TRANSVERSE SECTION

Dorsally on the left side are the sensory cells, and ventrally the motor cells. The processes of both sensory and motor cells branch into ascending and descending fibres, forming a sensory or afferent tract on the one hand and a motor or efferent tract on the other. On the left side of the figure the branching of a sensory fibre (*sf.*) to innervate both skin and muscle is illustrated dorsally, and ventrally a motor fibre (*mf.*) that innervates the muscle segment is shown with a branch going on to the primodium of the forelimb. (After Coghill, 1929.)

neurone (*n.*[2], fig. 16) so that sensation received from the ascending sensory tract of one side can now cross over to the descending motor path of the opposite side.

Establishment of primitive peripheral nerves

When the longitudinal mass of mesoderm is divided into myotomes (*m.*, fig. 15), then motor fibers leave axones of the motor tract and pass laterally toward the

active mesoderm to attach at the middle of the myotome (*m.*, figs. 15 and 16), where the nuclei of the myoblasts are located. It is assumed by Coghill that these peripheral motor root branches of the descending axones grow toward the region of higher metabolic activity.

Similarly, side branches as dendrites (*sf.*, figs. 15 and 16) grow off the sides of the sensory cells to form the primary peripheral sensory system. These branches give twigs to the skin and to the ends of the myotomes.

With the establishment of the peripheral nervous system, sensation received by the skin or from the myotomes passes over the afferent (sensory) nerve into the sensory tract, where it may either go forward toward the beginning brain or across by the commissure formed by the plate cells to the motor tract of the opposite side. If it takes the latter course, then from the motor tract it passes as a motor impulse over the efferent nerve to the myotomes, which at this time are capable of contraction.

Acknowledgment: I am indebted to the Cambridge University Press and the Macmillan Company for permission to use Figures 14, 15, and 16 from Coghill's *Anatomy and the Problem of Behavior*.

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STUDIES IN ANIMAL POPULATIONS

II. SEASONAL POPULATION-TRENDS OF THE HONEY-BEE

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1. GENERAL OBSERVATIONS ON HONEY-BEE POPULATIONS

THE honey-bee (*Apis mellifica* L.) is a social bee with populous colonies, which under natural conditions dwell in caves of old trees, in clifts of rocks, etc. The nests persist for many years and a formation of new colonies takes place by partition of the original colony. At the seasonal population peak a large percentage of the colony may swarm out together with, or followed by, the old or young queen. The swarming bees aggregate in a cluster and enter a new suitable breeding locality. Fertilization occurs only once in the lifetime of the queen (longevity: 3–4 years, maximum observed 9 years), during a special nuptial flight and the sperms of this copulation last throughout the queen's life. The workers are genotypically females which by special feeding have been bred to workers, whereas the diet of queen larvae is especially rich in vitamin E. The males originate from unfertilized eggs. Honey and pollen are stored during the favourable season. During the unfavourable season the brood-rearing cycle is interrupted in temperate regions and reduced in warmer climates.

In modern bee-hives (of the Langstroth-type) there are 30,000–40,000 and up to 70,000 adult bees present at the population peak. Most of these individuals are workers. The quantity of drones is very different, assumingly based mainly on racial differences, and ranges from 50 to 5,000 per colony. During the bad season (e.g. hibernation) the colony may be reduced to 10,000 worker bees or less. In a good hive about 30,000 bees may leave the hive with the first swarm. Eventual after-swarms are much less populous. The fertility of the queen has often been greatly exaggerated. For good colonies a daily maximum oviposition rate (maintained only during some weeks) of 1000–2000 eggs can be expected. From 100,000–150,000 eggs total annual production is expected from a good queen under favourable conditions. A general discussion of all these data may be found in Bodenheimer (1936).

2. EBERT'S ANALYSIS OF THE SEASONAL POPULATION TREND IN THE HIVE OF THE HONEY-BEE

The number of the exact census of bee-populations is very small and all of them content themselves by considering only part of the total populations: either brood, sealed brood only, or adults. Exact counts for over a whole season are only those made by Nolan (1925) of the sealed brood. But the population is composed of all ages and stages. Counts of human populations taking into consideration only the ages above or below 30 years or even only those of 20–30 years may be of interest, but they do not permit an analysis of the trend of the total population.

The only previous attempt to study the dynamics of the total bee-population, known to the author, is that of Ebert (1922). He divides the bee-population

into three categories: brood, young bees (not yet leaving the hive) and field bees.

(1) The determination of the brood quantity present on every date is made with the aid of a planimeter or by calculating the surface as an ellipse, using the formula: sum of the products of the various heights and breadths multiplied by 1.6. An example is given in Table 1.

As 3.9 cells are present per cm^2. the product $1339 \times 3.9 = 5223$ represents the number of brood present.

(2) The determination of the field bees is made by weighing. The door of the hive is opened at a certain hour of normal flight activity in the morning and closed again when the increase of weight shows that the bees are returning. Ten bees are supposed to weigh 1 gram. The difference in the weight at both moments gives the number of field bees. When 570 grams difference are obtained, then 5700 field-bees are supposed to be present. This method is by no means exact, but should be respected as a first approach to the problem.

(3) The determination of the young bees is made during the absence of the field bees by taking out all combs and stripping off, with a carbolized rag, the young bees, which remain in the hive. The combs are weighed and as the weight of all other parts of the hive is known, the remaining difference yields the weight of the young bees. Their number is calculated as in (2): difference 273 grams = 2730 young bees.

These observations are made at intervals of 21 days. A second series of observations with 21 days (= duration of brood development) interval may be intercalated.

TABLE 1

	COMB I	COMB II	COMB III	SUM
Height.....	15	18	21	
Breadth.....	11	14	20	
Product.....	165	252	420	$837 \times 1.6 = 1{,}339\ cm^2$.

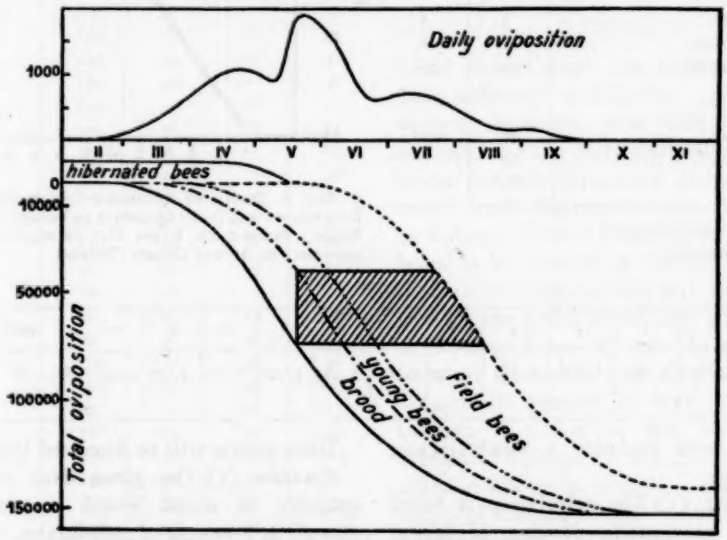

FIG. 1. THE SEASONAL TREND OF AN IDEAL BEE-POPULATION (AFTER EBERT)

This system is fairly exact for the total quantity of brood throughout the season. It yields both the total seasonal egg-production of the queen and, by additional calculation, the daily oviposition. An idealized scheme of the annual development is reproduced in Fig. 1 for a colony with 10,000 hibernated bees, 152,000 total egg-production from February to September and an average longevity of field bees of 7 weeks.

Many practical applications of this type of research are reported by Ebert, but his suggestions do not seem to have found listeners to this day.

3. A NEW METHOD OF CALCULATING THE SEASONAL DYNAMICS OF A HONEY-BEE-COLONY

Further research in the biology of bees, and especially the work of Nolan (1925), Roesch (1925) and Morland (1930) enable us now to use a more simple system in evaluating the population trend of a bee-colony with probably a much greater accuracy.

TABLE 2

	♀	♂
Development:	(*days*)	(*days*)
Egg-stage	3	3
Larval-stage	6	6
Sealed-brood	12	16
Adult life:		
Nurse-bee	10	100†
House-bee	10	
Field-bee	22*	

* (In winter longer.)
† (3–4 months.)

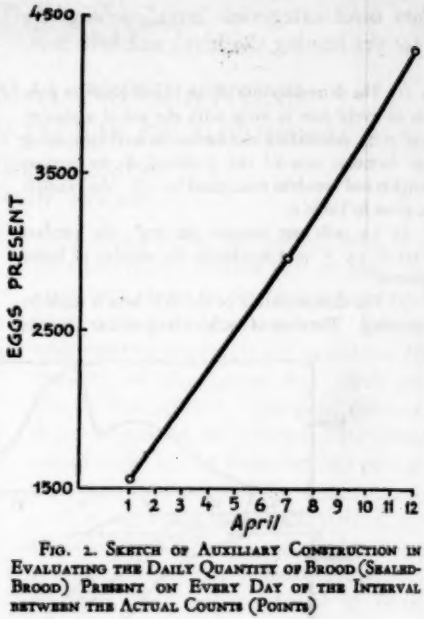

FIG. 2. SKETCH OF AUXILIARY CONSTRUCTION IN EVALUATING THE DAILY QUANTITY OF BROOD (SEALED-BROOD) PRESENT ON EVERY DAY OF THE INTERVAL BETWEEN THE ACTUAL COUNTS (POINTS)

TABLE 3

1	2	3	4	5	6	7	8	9	10	11	12	APRIL
1,553	1,810	2,060	2,300	2,550	2,790	*2,986*	3,280	3,520	3,770	4,010	*4,297*	No. of sealed brood

Premises: (1) The calculation is based on the consecutive division of labour (Roesch, Morland), which on the average results in the scheme shown in Table 2.

The stages of adult life are to a certain degree modifiable and the house-bees especially may serve somewhat longer as nurses, or be speeded to serve as field-bees. They are therefore called "control-bees" by some American authors. (2) Any successfully developing egg does not show under normal conditions a further mortality (cf. Bodenheimer, 1936). (3) The fertility of the queen does not differ considerably from one day to another, but changes only gradually.

These points will be discussed later on.

Procedure: (1) The given data on the quantity of sealed brood present are plotted in a system of coördinates, where the abscissa illustrates the time, the ordinate the number of sealed brood present. The dots of the observed data are joined by a line and the number of sealed brood present at any day of the interval between two dots can be read on the ordinate (cf. Fig. 2). The values for the intervals between the two observations of 1st and 7th and 12th of April are seen in Table 3.

(2) The second step is the evaluation of the daily addition for the stage concerned (sealed brood in our case). Four columns are erected for this purpose. Column I contains the quantity of sealed

TABLE 4

DATE	I SEALED BROOD PRESENT EVERY DAY	II DAILY DIFFERENCE AGAINST YESTERDAY	III DAILY CHANGE INTO NURSE-BEES	IV ACTUAL DAILY INCREASE OF SEALED BROOD
Feb.				
23	0	0	—	0
24	5	5	—	5
25	12	7	—	7
26	18	6	—	6
27	23	5	—	5
28	29	6	—	6
29	34	5	—	5
Mar.				
1	*41*	7	—	7
2	100	59	—	59
3	140	40	—	40
4	180	40	—	40
5	220	40	—	40
6	275	55	0	55
7	320	45	5	50
8	360	40	7	47
9	400	40	6	46
10	450	50	5	55
11	490	40	6	46
12	520	30	5	35
13	580	60	7	67
14	620	40	59	99
15	*660*	40	40	80
16	590	−70	40	−30
17	520	−70	40	−30
18	440	−80	55	−25
19	*368*	−72	50	−22
20	410	42	47	89
21	450	40	46	86
22	500	50	55	105
23	530	30	46	76
24	585	55	35	90
25	*608*	23	67	90
26	850	242	99	341
27	880	30	80	110
28	1,010	140	−30	110
29	1,160	140	−30	110
30	1,300	130	−25	105
31	*1,533*	123	87	210
Apr.				
1	1,810	257	86	343
2	2,060	250	105	355
3	2,300	240	76	316
4	2,550	250	90	340
5	2,790	240	90	330
6	*2,986*	196	341	537
7	3,280	294	110	404

TABLE 4—*Concluded*

DATE	I SEALED BROOD PRESENT EVERY DAY	II DAILY DIFFERENCE AGAINST YESTERDAY	III DAILY CHANGE INTO NURSE-BEES	IV ACTUAL DAILY INCREASE OF SEALED BROOD
Apr.				
8	3,520	240	110	350
9	3,770	250	110	360
10	4,010	240	105	345
11	*4,297*	287	118	406
12	4,460	163	210	373
13	4,630	170	343	512
14	4,810	180	355	535
.	.	.	.	.
.	.	.	.	.

brood present every day, Column II, the daily difference of Column I against yesterday. However, this difference does not represent the daily addition of sealed brood, because after every 12th day the sealed brood disappears daily, becoming nurse-bees. This disappearing sealed brood is represented in Column III, and Column IV represents the real daily addition of new sealed brood after the values in Columns II and III have been added. Columns III and IV are identical, only Column III appears 12 days later than Column IV. The 12 first values are entered in Column IV and Column IV is calculated as far as possible. The 12 last values of Column IV are then entered as a continuation of Column III, etc.

The calculation of the Cyprian colony (Nolan, 1926) may serve as an example: The actually published data on the quantity of sealed brood at various dates are reproduced in Nolan's Chapter 4. Table 4 illustrates the procedure hitherto described for getting the first steps of the calculation.

(3) In this way, we received the real daily addition of sealed brood. When every instar has a definite duration, this daily increase must be identical for all stages. We have to defer the dates of

TABLE 5

DATE	I DAILY INCREASE OF EGGS	II DAILY INCREASE OF LARVAE	III DAILY INCREASE OF SEALED BROOD	IV DAILY INCREASE OF NURSE-BEES	V DAILY INCREASE OF HOUSE-BEES	VI DAILY INCREASE OF FIELD-BEES
Feb.						
14	0	—	—	—	—	—
15	5	—	—	—	—	—
16	7	—	—	—	—	—
17	6	0	—	—	—	—
18	5	5	—	—	—	—
19	6	7	—	—	—	—
20	5	6	—	—	—	—
21	7	5	—	—	—	—
22	59	6	—	—	—	—
23	40	5	0	—	—	—
24	40	7	5	—	—	—
25	40	59	7	—	—	—
26	55	40	6	—	—	—
27	50	40	5	—	—	—
28	47	40	6	—	—	—
29	46	55	5	—	—	—
Mar.						
1	55	50	7	—	—	—
2	46	47	59	—	—	—
3	35	46	40	—	—	—
4	67	55	40	—	—	—
5	99	46	40	—	—	—
6	80	35	55	0	—	—
7	−90	67	50	5	—	—
8	−30	99	47	7	—	—
9	−25	80	46	6	—	—
10	−22	−30	55	5	—	—
11	87	−30	46	6	—	—
12	86	−25	35	5	—	—
13	105	−22	67	7	—	—
14	76	87	99	59	—	—
15	90	86	80	40	—	—
16	90	105	−30	40	0	—
17	341	76	−30	40	5	—
18	110	90	−25	55	7	—
19	110	90	−22	50	6	—
20	110	341	87	47	5	—
21	105	110	86	46	6	—
22	118	110	105	55	5	—
23	210	110	76	46	7	—

TABLE 5—*Concluded*

DATE	I DAILY INCREASE OF EGGS	II DAILY INCREASE OF LARVAE	III DAILY INCREASE OF SEALED BROOD	IV DAILY INCREASE OF NURSE-BEES	V DAILY INCREASE OF HOUSE-BEES	VI DAILY INCREASE OF FIELD-BEES
Mar.						
24	343	105	90	35	59	—
25	355	118	90	67	40	—
26	316	210	341	99	40	0
27	340	343	110	80	40	5
28	330	355	110	−30	55	7
29	537	316	110	−30	50	6
30	404	340	105	−25	47	5
31	350	330	118	−22	46	6
Apr.						
1	360	537	210	87	55	5
2	345	404	343	86	46	7
3	405	350	355	105	35	59
4	373	360	316	76	67	40
5	513	345	340	90	99	40
6	535	405	330	90	80	40
7	506	373	537	341	−30	55
8	530	513	404	110	−30	50
9	530	535	350	110	−25	47
10	740	506	360	110	−22	46
	.	.	.	.	.	.
	.	.	.	.	.	.

sealed brood additions for 12, 20 or 30 days in order to receive the daily addition of nurse-, house-, and field-bees respectively. In postponing the additions for another 22 days, we evaluate the normal longevity of the field-bees. This system of book-keeping is shown in Table 5.

(4) Now everything is prepared for the final calculation. For most purposes it will be quite sufficient to know the number of all brood-instars present in a hive in weekly intervals. With respective changes the same calculation may be applied for smaller or longer intervals.

The last day before the beginning of the seasonal oviposition is marked as starting point. Every seventh day of the last table

TABLE 6.—*Weekly status of a bee-population (Cyprus-race, Nolan 1926)*

DATE	I EGGS 3	II LARVAE 6	III SEALED BROOD 11	IV NURSE-BEES 10	V HOUSE-BEES 10	VI FIELD-BEES 22	VII DRONE EGGS 3	VIII DRONE LARVAE 6	IX DRONE SEALED-BROOD 16	X DRONE ADULTS 100	XI TOTAL LIVING
Feb.											
14	0	0	0	0	0	0	0	0	0	0	0
21	18	23	0	0	0	0	0	0	0	0	41
28	152	191	29	0	0	0	0	0	0	0	372
Mar.											
6	246	279	275	0	0	0	0	0	0	0	800
13	278	82	580	41	0	0	0	0	0	0	981
20	330	788	408	349	23	0	0	0	0	0	1,898
27	1,011	996	868	580	215	5	0	0	0	0	3,675
Apr.											
3	1,111	2,977	2,058	417	454	100	0	0	0	0	7,116
10	1,800	2,677	3,768	1,205	285	418	3	6	0	0	10,162
17	2,031	3,246	5,188	2,117	808	770	20	14	7	0	14,201
24	3,448	5,021	6,249	3,742	1,697	1,046	158	46	23	0	21,360
May											
1	3,899	6,619	8,818	4,837	3,303	1,670	111	322	122	5	29,706
8	3,909	8,062	12,788	5,807	4,318	3,264	90	202	470	20	38,930
15	3,053	6,515	15,298	9,013	5,330	5,857	471	591	654	64	46,840
22	3,308	6,550	14,357	11,774	7,030	8,865	32	728	1,060	388	54,092
29	3,811	6,340	12,494	13,229	10,615	11,089	362	126	1,475	681	60,222
June											
5	3,389	7,414	13,187	10,998	12,788	15,642	—	255	1,378	1,168	60,219
12	3,527	7,179	13,937	10,749	12,266	20,381	438	—	342	2,099	70,919
19	3,076	7,155	16,517	11,168	10,121	25,061	—	483	450	2,297	76,328
26	3,804	7,424	14,197	11,837	10,507	24,170	244	82	362	2,865	75,492
July											
3	3,345	6,930	13,327	11,738	11,817	23,359	—	546	747	2,575	74,394
10	2,923	6,927	13,817	11,302	11,940	22,027	123	—	412	3,308	72,779
17	2,712	4,808	13,056	12,137	11,996	23,098	—	515	476	3,235	72,032
24	2,061	5,092	11,216	11,440	12,082	23,799	122	—	531	3,764	70,107
31	2,604	4,322	8,916	10,525	11,980	24,116	37	342	484	3,700	68,026
Aug.											
7	2,014	5,120	9,416	8,493	10,891	24,373	350	—	557	4,155	65,369
14	2,656	4,039	9,596	8,121	9,282	23,594	—	349	502	4,229	62,368
21	3,031	4,656	9,766	7,724	8,164	23,107	235	389	470	4,420	61,962
28	2,884	6,317	10,154	8,082	7,641	20,529	298	125	859	4,203	61,152
Sept.											
4	1,456	4,434	11,061	8,164	8,689	18,823	—	575	467	4,470	68,139
11	2,071	3,214	9,916	8,581	7,872	17,768	270	204	831	4,382	55,309
18	2,446	4,684	6,418	10,299	8,086	17,584	141	12	890	3,639	54,199
25	3,067	4,893	8,016	6,642	9,371	18,275	—	526	327	3,847	54,964
Oct.											
2	210	1,417	9,646	5,631	8,442	18,724	258	—	860		45,191
9	—	800	8,516	7,686	5,247	19,984	—	258	675		43,166
16		70	2,962	7,632	6,800	17,521		—	356		35,341

TABLE 6.—Concluded

DATE	I EGGS 3	II LARVAE 6	III SEALED BROOD 12	IV NURSE-BEES 10	V HOUSE-BEES 10	VI FIELD-BEES 22	VII DRONE EGGS 3	VIII DRONE LARVAE 6	IX DRONE SEALED-BROOD 16	X DRONE ADULTS 100	XI TOTAL LIVING
Oct.											
23*		—	760	5,277	7,766	16,125			—		29,928
30			—	2,069	7,724	14,038					23,831
Nov.											
6				—	1,389	16,825					
13					632	13,279					
20					—						

* From October 23, all stages are regarded as stable. The total number of 29,928 bees is greatly reduced at the beginning of winter and presumably to a lesser degree during the winter. From 10,000 to 15,000 adult bees normally survive until the coming brood-rearing cycle. These survivors die at a rapid rate during the first weeks of brood-rearing.

is underlined and the following additions made: for evaluating the number of eggs present at any of these dates the daily additions to the egg-stage for the last 3 days are summed up (duration of egg stage, 3 days). The respective additions for the other instars are: larvae six daily increases, sealed brood 12 days, nurse-bees 10 days, house-bees 10 days and field-bees 22 days.

As in the present case, the sealed brood for workers and drones had been registered separately, we have to perform the calculation separately too. The relative data for drones are: egg 3 days, larva 6 days, sealed brood 16 days, adult drones 100 days (3–4 months, until the drone-battle). In this way the final table (Table 6) is obtained.

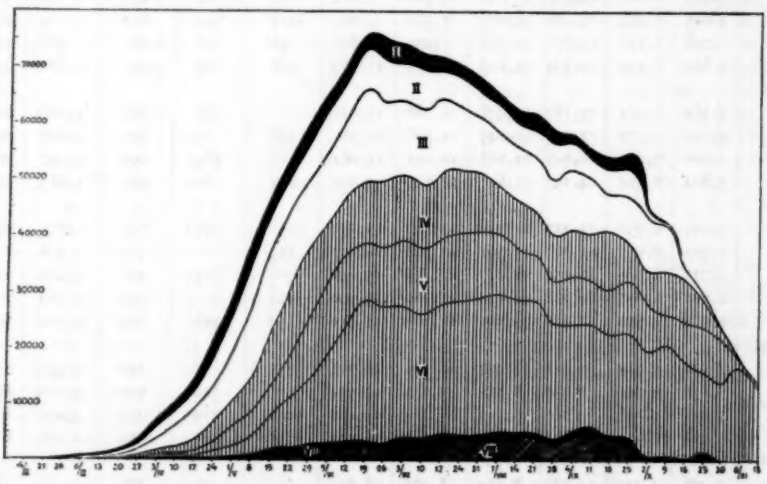

FIG. 3. THE SEASONAL POPULATION TREND OF A COLONY OF CYPRIAN BEES (1926)
(Calculated after Nolan's observations on sealed brood)

I–VI, workers; VII/VIII, drones. I, egg; II, larvae; III, nymphs; IV, nurse-bees; V, house-bees; VI, field-bees. VII, drone brood; VIII, adult drones.

In our own observations we contented

ourselves with the total number of brood (all instars) present at a 21 day interval. Following the procedure of Ebert, improved by Polteff, we measured the maximal height and breadth of the brood area of both sides of every comb in units of 16 mm. It is then possible to read from a table prepared by Polteff the number of cells covered by the brood area without further calculation.

of empty cells present in the brood area. In two cases for example the readings were:

Units read	*Polteff's table*	*Actual brood*	*Empty cells*	*Total*
8 x 15	900	944	81	1,025
9 x 18	1,300	1,365	44	1,409

Also negative deviations were observed. It may therefore be assumed that the quick

TABLE 7

Polteff's table

(The units of length and height are 16 mm. each, those of the combination hundreds [3 = 300].)

LENGTH	HEIGHT																			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	—	—	—	—	—	—	—	1	1	1	1	1	1	1	1	1	1	1	1	2
2	—	—	—	1	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3
3	—	—	1	1	1	1	2	2	2	2	3	3	3	3	3	4	4	4	4	5
4	—	1	1	1	2	2	2	2	3	3	3	4	4	4	5	5	5	6	6	6
5	—	1	1	2	2	2	3	3	3	4	4	5	5	5	6	6	7	7	7	8
6	—	1	1	2	2	3	3	4	4	5	5	6	6	7	7	7	8	8	9	9
7	—	1	2	2	2	3	4	4	5	5	6	7	7	8	8	9	9	10	10	11
8	1	1	2	2	3	4	4	5	6	7	7	7	8	9	9	10	11	11	12	13
9	1	1	2	3	3	4	5	5	6	7	8	8	9	10	11	11	12	13	13	14
10	1	2	2	3	4	5	5	6	7	8	9	9	10	11	12	13	13	14	15	16
11	1	2	3	3	4	5	6	7	8	9	9	10	11	12	13	14	15	15	16	17
12	1	2	3	4	5	6	7	7	8	9	10	11	12	13	14	15	16	17	18	19
13	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
14	1	2	3	4	5	7	8	9	10	11	12	13	14	15	16	18	19	20	21	22
15	1	2	3	5	6	7	8	9	11	12	13	14	15	16	18	19	20	21	22	24
16	1	2	4	5	6	7	9	10	11	13	14	15	16	18	19	20	21	23	24	25
17	1	3	4	5	7	8	9	11	12	13	15	16	17	19	20	21	23	24	25	17
18	1	3	4	7	7	8	10	11	13	14	15	17	18	20	21	23	24	25	27	28
19	1	3	4	6	7	9	10	12	13	15	16	18	19	21	22	24	25	27	28	30
20	2	3	5	6	8	9	11	13	14	16	17	19	20	22	24	25	27	28	30	31
21	2	3	5	7	8	10	11	13	15	16	18	20	21	23	25	26	28	30	31	33
22	2	3	5	7	9	10	12	14	15	17	19	21	22	24	26	28	29	31	33	35
23	2	4	5	7	9	11	12	14	16	18	20	22	23	25	27	29	31	32	34	36
24	2	4	6	7	9	11	13	15	17	19	21	23	24	26	28	30	32	34	36	38
25	2	4	6	8	10	12	14	16	18	20	22	24	25	27	29	31	33	35	37	39

While in Polteff's hive 800 cells were present in 200 cm.², in some counts in Palestine there were 806, 812, and 836. We would thus obtain a number of cells of the brood area, which is somewhat below their actual number. However, this deviation is balanced by the number estimate gained by this method leads to figures approaching ± 5 per cent of the actually present brood. This approximation is sufficiently significant to justify the application of this quick and simple method.

Polteff's table, having appeared in a not

easily accessible journal, is given here for the benefit of other workers (Table 7).

TABLE 8

Nolan's records of sealed brood in some bee colonies throughout the year

(The dates are the days of inspection, the figures the number of sealed brood present in the hive on the day of inspection.)

ITALIAN RACE		CYPRIAN RACE		
Colony 4 (1921)	Sealed brood	(1926)	Workers	Drones
Mar.		Mar.		
16	5,012	1	41	—
23	9,024	15	660	—
29	11,186	19	368	—
Apr.		25	608	—
5	12,327	Apr.		
12	14,787	1	1,553	—
19	15,402	7	2,986	—
26	14,072	12	4,297	—
May		19	5,593	9
3	15,028	24	6,249	23
10	16,982	30	8,283	66
17	17,859	May		
24	17,155	7	12,240	431
31	10,955	12	14,710	615
June		18	15,737	661
7	13,079	24	13,499	1,270
14	12,408	29	12,387	1,575
21	13,674	June		
28	14,588	8	13,426	1,180
July		14	13,949	215
5	13,659	21	14,127	370
12	11,741	28	13,925	343
19	11,084	July		
26	9,904	8	14,375	33
Aug.		16	13,186	24
2	8,204	26	10,548	18
9	7,840	Aug.		
16	7,459	2	9,472	17
23	7,871	9	9,133	10
30	8,896	15	9,430	22
Sept.		28	9,931	16
6	9,448	Sept.		
13	8,015	7	11,246	229
20	7,862	18	5,702	17
27	9,433	Oct.		
Oct.		6	9,869	5
4	7,644	15	3,648	—
11	3,973			
18	1,143			

4. THE SEASONAL HISTORY OF NOLAN'S HIVES AT BALTIMORE

The data calculated here (Table 8) are based on Nolan's counts of sealed brood in a large apiary near Baltimore. These counts were made at (not less than) 12 day intervals on colonies of different races and quality.

We shall describe briefly the conditions of development in Nolan's model colony No. 4, 1921 (Table 9), which proved to be identical in two successive years. This colony stored the most surplus honey. It was left in the preceeding winter without packing, in two 10-frame Langstroth hive bodies with abundant stores of honey. In the spring, this colony started with a fairly strong force of hibernated bees, a prolific queen, combs composed chiefly of worker cells and no shortage of honey stores.

1. Initial expansion. The chief source of nectar, tulip tree, was yielding freely by May 21. The maximum of sealed brood was reached during the third week in May. Nectar and pollen came in abundantly throughout March, but the secretion of nectar and the production of pollen were affected adversely by the inclement weather at the end of March and beginning of April, and probably the lower temperatures also affected unfavorably the activity of the unpacked colony. The adverse conditions finally checked the initial seasonal expansion of the brood, as is shown by a decrease in the amount of sealed brood in the latter half of April. A recovery in the rate of brood rearing was made subsequently, but at a time when factors associated normally with the major period were making themselves felt. It follows that the maximum amount of brood rearing in 1921 was nor purely the result of the initial seasonal tendency.

2. Major period. Since the maximum had been attained just before the locust bloom, the high rate was kept up for a couple of weeks. During the week of maximum sealed brood, brood rearing was undoubtedly still being carried on under the impulse of the initial tendency, but influencing factors characteristic of the major period were also becoming evident. The week of the maximum marked the point of division between the initial seasonal tendency and the major period. Nectar subsequently

TABLE 9

Italian bee-colony 4 (1921)

DATE	I EGGS (3)	II LARVAE (6)	III SEALED BROOD (12)	IV NURSE-BEES (10)	V HOUSE-BEES (10)	VI FIELD-BEES (12)	VII TOTAL
Feb.							
11	—	—	—	—	—	—	—
18	100	40	—	—	—	—	140
25	400	405	70	—	—	—	875
Mar.							
4	1,710	1,320	600	—	—	—	3,640
11	2,245	3,970	2,230	140	—	—	8,585
18	2,810	5,410	6,075	840	40	—	15,185
25	2,595	5,980	9,195	3,160	475	5	21,410
Apr.							
1	4,291	6,625	11,195	6,695	1,710	200	30,716
8	3,482	8,510	12,925	8,315	5,175	1,195	39,602
15	4,645	7,512	14,565	9,535	7,590	4,285	48,132
22	4,518	6,038	14,305	11,811	9,620	8,255	54,543
29	3,627	8,578	13,925	11,997	10,758	14,097	61,982
May							
6	4,420	8,358	16,365	12,072	12,336	18,814	71,365
13	3,721	7,370	16,885	12,100	11,722	22,150	73,948
20	2,024	4,787	17,175	13,403	11,154	24,130	72,673
27	5,515	8,313	14,235	14,220	13,060	26,475	81,818
June							
3	544	2,433	11,475	13,776	14,035	25,984	68,227
10	4,152	9,522	12,425	10,244	15,015	27,373	78,731
17	2,896	2,924	12,555	10,690	11,779	30,129	70,973
24	1,165	10,202	12,501	9,520	8,238	30,279	71,905
July							
1	4,838	2,122	11,481	11,098	11,774	26,835	68,148
8	852	6,332	11,204	8,882	9,035	26,627	62,932
15	3,787	3,743	9,504	11,480	11,356	20,936	60,806
22	1	2,465	8,695	6,692	11,768	24,406	54,026
29	599	6,697	7,310	8,615	7,800	21,362	52,383
Aug.							
5	2,530	−613	6,160	7,982	8,744	24,176	48,979
12	221	5,698	5,780	4,040	5,450	21,111	42,300
19	4,686	1,207	5,645	5,824	7,645	17,342	41,349
26	318	6,107	7,615	2,600	5,446	16,172	37,622
Sept.							
2	1,972	4,738	7,129	6,405	4,387	11,184	35,815
9	1,216	644	7,583	5,824	5,424	13,404	34,095
16	619	6,712	5,918	5,786	3,361	9,767	32,183
23	0	1,054	6,528	6,033	6,952	11,627	32,194
30	0	0	7,985	3,179	4,456	12,342	29,978
Oct.							
7	0			6,677	5,617	11,362	29,631

From October 7 on, all adult bees remain in their respective stages of development. A strong initial and a less strong later hibernal mortality reduces them to 10,000–15,000 adults at the beginning of the following breeding season.

coming in from the tulip-tree tended to restrict the queen, and after this honey flow there was a dearth of nectar until the middle of September. During June, however, there was an appreciable amount of incoming pollen, and in August there was an intense pollen yield. As a consequence the decline in brood-rearing activity which set in with the beginning of the tulip-tree honey flow and extended until the intense pollen yield in August was broken by a marked increase in June, in response to the pollen yield. The sharp decline which proceeded it happened because just prior to the honey flow the queen had ascended to the third hive body. Incoming nectar, however, so quickly cut down the number of cells available for the queen as to force her soon to return

3. Final contraction. The final contraction in brood-rearing activity took place almost entirely in October, covering three weeks. The abruptness of the contraction in these few weeks is shown from the fact that in the last week of September there were practically half as many cells of sealed brood as were found in the maximum counts for the year. As a result, the colony entered the following season strong in bees.

4. Year 1922. The seasonal brood-rearing curve is strikingly similar to that in 1921, taking into consideration certain minor differences due primarily to weather conditions.

5. Conclusions. The brood-rearing record of this colony, although not ideal, is the most satisfactory of

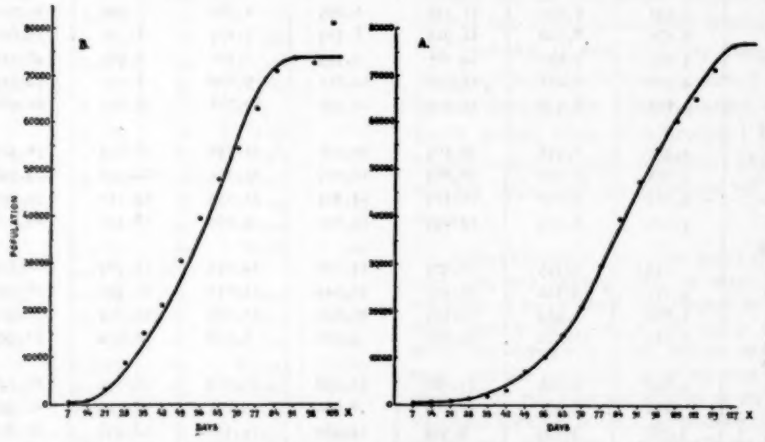

FIG. 4. THE GROWTH CURVE (LOGISTIC CURVE) OF A COLONY OF CYPRIAN (A) AND ITALIAN (B) BEES NEAR BALTIMORE
(Calculated from the data obtained by Nolan)

to the second hive body. Here, too, so many available cells within the brood area proper had been filled with nectar during her absence that the total number of cells made empty either by emerging bees or by consumption of stores did not suffice during that week to permit keeping up her former rate. During the next two weeks, with the emergence of many bees in both the second and third hive bodies, the queen had more room. There was also an exceptionally large quantity of honeydew available, associated with incoming pollen during June, also a certain amount of nectar from sweet clover. Throughout this month the queen had all the room needed for a normal response to these stimuli. At the end of July brood-rearing activity had been reduced to approximately one-half that represented by the maximum.

any of the 16 (studied) colonies because the maximum brood rearing bears some correlation to the initial expansion. The portion of the major period immediately following the period of main nectar secretion is not marked by a disproportionate degree of brood-rearing activity. In the late stages of the major period, moreover, there is an increase in brood rearing, providing a sufficient number of newly emerged bees at the beginning of the final contraction to insure successful wintering and an auspicious beginning of the next active season. That conditions within the colony remained nearly constant during the two consecutive years is indicated by the striking similarity in the curves of brood-rearing activity during both active seasons (Fig. 4). The performance of this colony leads to the conclusion that,

other conditions being equal, a strong colony tends to remain strong. (Nolan, 1925.)

It is of interest to compare the brood-rearing cycle of a colony of Cyprian bees in 1926 at the same locality in Maryland (Nolan, 1928). It proved to be strikingly similar to the typical colony of Italian bees which has just been described (Colony 4). Other singularities which are ascribed in literature to the Cyprian race did not verify. The colony exhibited no tendency to tolerate laying worker-bees while it had a laying queen, nor did it build more queen cells than usual for the Italian bees in the same apiary. The queen was not more prolific than that of the Italian race.

This statement is highly important, because it shows that differences in behavior, fertility, etc., which generally are described as racial qualities, are in reality responses to certain environmental conditions in the same direction or even intensity which other races of the honey bee show towards the same conditions.

The development of Nolan's representative colonies (Cyprian 1926, Italian 1921) is computed and compiled in Tables 10 and 11 respectively. x indicates the time in days from the beginning of the seasonal population increase, y the total bee-population of all ages and stages, and k the maximum reached (upper asymptote).

TABLE 10

x	y	$k-y$	$\frac{k-y}{y}$	$\log\frac{k-y}{y}$
0	0	76,328	∞	∞
7	41	76,287	1,860.66	3.26975
14	372	75,956	204.16	2.30984
21	800	75,528	94.41	1.97502
28	981	75,347	76.81	1.88942
35	1,898	74,430	39.21	1.59340
42	3,675	72,653	19.76	1.29579
49	7,116	69,212	9.72	0.98767
56	10,162	66,166	6.51	0.81358
63	14,201	62,127	4.37	0.64048
70	21,360	54,968	2.57	0.40993
77	29,706	46,622	1.55	0.19033
84	38,930	37,398	0.96	0.99127−1
91	46,846	29,482	0.63	0.79934−1
98	54,092	22,236	0.41	0.61278−1
105	60,222	16,106	0.26	0.41497−1
112	66,219	10,109	0.15	0.17609−1
119	70,919	5,409	0.07	0.84510−2
126	76,328	0	0	

$x = 21.$ $\log\frac{k-y}{y} \times 2.3026 = 1.97502 \times 2.3026 = 2.5726.$

$x = 77.$ $\log\frac{k-y}{y} \times 2.3026 = 0.19033 \times 2.3026 = 0.2479.$

$a + 21\,b = 2.5726.$
$a + 77\,b = 0.2479.$

$b = -0.2906.$
$a = 3.4444.$

$$y = \frac{76.328}{1 + e^{3.4444 - 0.2906x}}.$$

TABLE 11

x	y	$k-y$	$\frac{k-y}{y}$	$\log\frac{k-y}{y}$
0	0	74,000		
7	140	73,860	527.1	2.72189
14	875	73,125	83.5	1.92169
21	3,640	70,360	19.3	1.28556
28	8,585	65,415	7.61	0.88138
35	15,185	58,815	3.87	0.58771
42	21,910	52,590	2.46	0.42160
49	30,726	43,274	1.42	0.14922
56	39,602	34,398	0.86	0.93450−1
63	48,132	25,868	0.52	0.71600−1
70	54,543	19,457	0.35	0.54407−1
77	62,982	11,018	0.17	0.23045−1
84	71,365	2,635	0.03	0.47712−2
91	73,948	52	0.001	0

$x = 28.$ $\log\frac{k-y}{y} = 0.88138 \times 2.3026 = 1.9195.$

$x = 77.$ $\log\frac{k-y}{y} = 0.23045 - 1 \times 2.3026 = -1.7618.$

$a + 28\,b = 1.9295$
$a + 77\,b = -1.7618$

$b = -0.075$
$a = 4.0295$

$$y = \frac{74,000}{1 + e^{4.0295 - 0.075x}}.$$

The seasonal development of the Cyprian bee follows a perfect logistic curve. This is calculated from Table 10: $k = 76{,}328$.

The agreement between the simple formula of the logistic curve and the population trend is not as good in the development of the Italian bee colony No. 4 in 1921. This discrepancy is fully explained by Nolan's explanation under "Initial expansion." The conditions prevailing in March and April were not very favourable and would have allowed a maximal development of only about 74,000 individuals. The very favourable conditions starting in the latter half of May produced an additional rise, which superseded the initial seasonal tendency. This superposition increased the maximum to about 82,000 individuals. The influence of environmental conditions on the population is thus clearly demonstrated.

The data for this colony are in Table 11: $k = 74{,}000$.

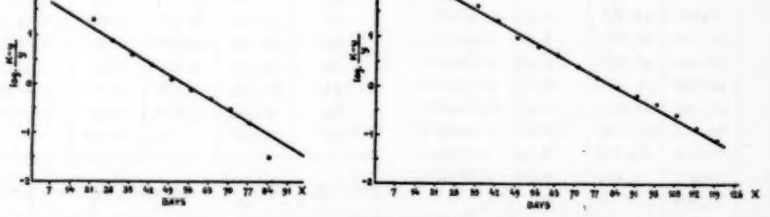

FIG. 5. THE LOGARITHMIC FITTING WITH THE STRAIGHT LINE FOR THE SEASONAL POPULATION GROWTH OF THE COLONIES DESCRIBED IN FIG. 4

5. THE SEASONAL HISTORY OF A BEE-HIVE NEAR JERUSALEM

The seasonal history of a modern bee hive (type Langstroth, single-walled) at Kiryath Anavim in 1935/36 has been described in detail (Bodenheimer and Ben Nerya, 1936) elsewhere. Briefly summed up, it is as follows:

The most outstanding fact is the reduction of the interruption of the brood-rearing cycle to 19 days (October 19 to November 7), whereas in the warmer climate of the coastal plain no interruption at all is observed in a normal winter. The lowest population level is reached just at the end of December. The peak is gained rapidly at the end of April. The progressive part of the seasonal trend lasts thus four months. Oviposition decreases at first slowly from the beginning of April, later quickly, reaching its minimum in late June. This summer depression of the oviposition is manifested in a depression in the total population in late July, which shows a minor peak in September. From the interruption of oviposition in late October on, the total population dwindles quickly until the minimum is reached at the end of the year. The maximum and minimum population in April and December respectively show a relation of 9:1.

The flight activity reaches its peak in March/May, the egg-production from February to May, the quantity of stored honey and the maximal weight in April, the sources of nectar from March to June.

The population trend is summed up in Table 12.

In the Palestinian hills oviposition is interrupted only during a very short period, which lasted in 1935 from October

TABLE 12—*Population of Single Walled Hive 1935/36*

DATE	EGGS	LARVAE	PUPAE	NURSE-BEES	HOUSE-BEES	FIELD-BEES	TOTAL
Jan.							
1	1,315	2,650	2,500	1,240	1,655	2,440	11,800
11	1,915	2,680	5,170	1,900	1,240	3,245	16,150
21	1,735	3,590	5,940	4,340	1,900	4,115	21,620
Feb.							
1	2,815	3,520	7,610	5,010	4,755	3,710	27,430
11	2,835	5,690	8,440	5,790	5,010	6,780	34,545
21	3,225	5,580	11,400	6,430	5,270	9,110	41,015
Mar.							
1	3,695	7,360	11,300	9,470	6,320	10,460	48,605
11	3,045	7,290	13,410	8,280	9,470	12,440	53,935
21	3,125	6,170	13,410	11,850	8,280	18,490	61,355
Apr.							
1	2,780	6,240	12,070	11,470	12,090	20,330	65,980
11	2,975	5,970	11,770	10,010	11,470	23,230	65,325
21	2,925	6,150	11,350	10,390	10,010	25,460	66,285
May							
1	2,995	5,320	10,725	10,390	10,390	23,940	63,760
11	2,950	5,345	11,000	9,810	10,390	21,520	61,015
21	2,925	5,240	11,420	9,130	9,810	21,845	60,370
June							
1	1,590	5,155	11,410	9,180	8,890	21,690	57,915
11	1,595	2,340	9,500	9,440	9,180	21,960	54,015
21	650	2,540	5,695	8,345	9,440	20,095	46,765
July							
1	1,870	3,255	4,770	4,605	8,345	20,050	42,895
11	1,185	3,450	6,340	4,130	4,605	19,520	39,230
21	2,270	3,575	6,210	5,075	4,130	10,555	31,815
Aug.							
1	1,545	4,150	7,660	5,210	5,615	8,485	32,660
11	1,970	4,305	7,530	6,145	5,210	9,975	35,135
21	1,505	3,540	7,765	6,780	6,145	11,255	36,990
Sept.							
1	1,700	3,735	6,500	6,665	6,390	12,865	37,855
11	1,215	3,100	6,740	5,805	6,665	14,165	37,690
21	1,425	2,870	5,750	5,490	5,805	13,665	35,005
Oct.							
1	1,055	2,675	5,035	5,160	5,490	13,460	32,875
11	1,650	2,440	4,825	4,575	5,160	12,525	31,175
21	0	2,395	4,790	4,365	4,575	11,510	27,665

Correction: Beginning with October 22, the adults of all groups live 14 days longer than normal and all die between December 1–21 (nurse-bees 1, house-bees 11, field-bees 21 days), i.e. all groups agree again with the above scheme on December 21.

Nov.							
1	0	0	4,005	4,310	4,320	10,425	23,070
11	470	750	0	2,645	4,310	9,085	17,270
21	380	790	1,370	0	2,645	9,375	14,560
Dec.							
1	725	810	1,560	1,100	0	7,700	11,895
11	365	760	1,900	1,310	1,100	4,560	10,005
21	1,015	780	1,500	1,630	1,310	1,150	7,345

TABLE 12.—*(Corrected as above)*

DATE	EGGS	LARVAE	PUPAE	NURSE-BEES	HOUSE-BEES	FIELD-BEES	TOTAL
Nov.							
1	0	0	4,005	4,545	4,175	10,200	22,925
11	470	750	0	7,190	4,175	10,200	22,785
21	380	790	1,370	4,615	4,175	9,475	20,805
Dec.							
1	725	810	1,560	1,120	7,605	4,685	16,505
11	365	760	1,900	1,320	1,120	8,880	14,345
21	1,015	780	1,500	1,630	1,370	1,100	7,345

19 to November 7, i.e. 19 days. On our coast-plains such an interruption does not, as a rule, occur. But even so, the difference between the upper and lower population asymptote is rather remarkable: 66,285 − 7,345 = 58,940. Its sizes at minimum and maximum are 1:9, whereas the reproductive capacity (1 queen) remains constant. The environmental conditions, especially those of the vegetation, constitute the limiting factor.

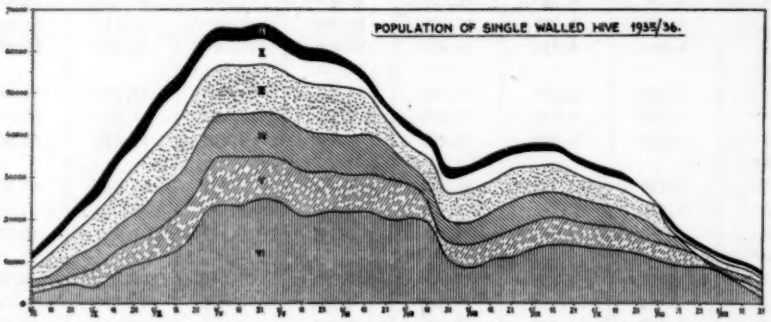

FIG. 6. THE SEASONAL POPULATION TREND OF A NORMAL COLONY OF A SYRIAN BEE (SINGLE WALLED HIVE) NEAR JERUSALEM

I, eggs; II, larvae; III, nymph; IV, nurse-bees; V, house-bees; VI field-bees (drones neglected).

TABLE 13

x	y	$y-d$	$k-(y-d)$	$\frac{k-(y-d)}{y-d}$	$\log\frac{k-(y-d)}{y-d}$
0	7,345	0	66,285	∞	∞
11	11,800	4,455	61,830	13.9	1.14301
21	16,150	8,805	57,480	6.5	0.81291
31	21,620	14,275	52,010	3.6	0.55630
41	27,430	20,085	46,200	2.3	0.36173
51	34,545	27,200	39,085	1.4	0.14613
61	41,015	33,670	32,615	0.96	0.98227−1
70	48,605	41,260	29,025	0.61	0.78533−1
80	53,935	46,590	19,695	0.42	0.62325−1
90	61,355	54,010	12,275	0.22	0.34241−1
101	65,980	58,635	7,650	0.13	0.11394−1
111	65,325	57,980	8,305	0.14	0.14613−1
121	66,285	58,940	7,345	0.12	0.07918−1

$$y = 7{,}345 - \frac{66{,}285}{1 + e^{2.8461 - 0.0464x}}.$$

In a parallel observation in a double-walled hive the population was as follows: k = 65,945; d = 6,400 (Table 14).

The good agreement between both formulae as well as of the general population trend shows that the observations may be regarded as typical for the specific year and locality. The interruption of oviposition in the double-walled hive

lasted from October 1 to November 11, i.e. 41 days.

Deviation of the calculated population trend from the observed one

Our observations are based on a series of counts made at about a 21 day interval. The brood quantities observed at this interval are connected by straight lines and these lines permit a fairly accurate estimate of the total brood present in the hive at every day of the year. The graphic results are now compared at certain intervals with those of our dynamic calculations. The absolute and percentage ± deviations from the graphical observation are indicated in Table 15.

TABLE 14

x	y	$y-d$	$k-(y-d)$	$\frac{k-(y-d)}{y-d}$	$\log\frac{k-(y-d)}{y-d}$
0	6,400	0	65,945	∞	∞
11	13,810	7,410	58,505	7.9	0.89763
21	20,830	14,430	51,515	3.6	0.55630
31	28,050	21,650	44,295	2.0	0.30103
41	36,680	30,280	35,665	1.1	0.04139
52	42,310	35,910	30,035	0.84	0.92428−1
62	47,740	41,340	24,605	0.59	0.77085−1
70	53,300	46,900	19,045	0.41	0.61278−1
80	58,530	52,130	13,815	0.26	0.41497−1
90	63,690	57,290	8,655	0.15	0.17609−1
101	65,945	59,545	6,400	0.11	0.04139−1

$$y = 6400 - \frac{65{,}945}{1 + e^{2.2154 - 0.0445x}}.$$

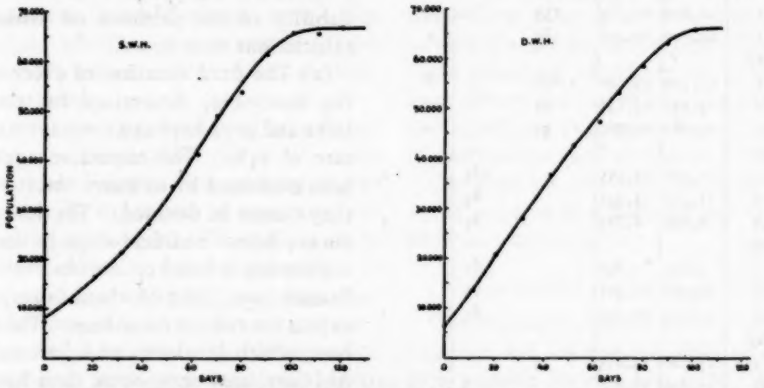

FIG. 7. THE GROWTH CURVE (LOGISTIC CURVE) OF A COLONY (A) SINGLE WALLED HIVE, (B) DOUBLE WALLED HIVE OF THE SYRIAN BEE NEAR JERUSALEM

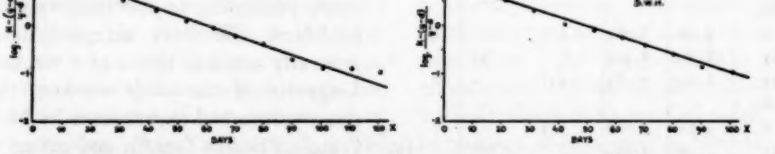

FIG. 8. THE LOGARITHMIC FITTING WITH THE STRAIGHT LINE FOR THE SYRIAN BEE (A) SINGLE WALLED HIVE, (B) DOUBLE WALLED HIVE

The average total deviations of the single walled hive are − 6045 = 1.3 per cent, and + 3670 = 0.8 per cent respectively. Their sum is 2.1 per cent, their difference 0.5 per cent.

It is obvious that our method of cal-

TABLE 15

DATE	TOTAL BROOD		DEVIATION			
	Graph	Calculated	Absolute		Percentage	
Jan.						
1	6,600	6,465	−135	+	−2	+
11	9,700	9,765		65		1
21	11,200	11,265		65		1
Feb.						
1	13,250	13,955		705		5
11	16,900	16,965		65		—
21	20,350	20,205	145		1	
Mar.						
1	22,500	22,355	145		1	
11	24,700	23,745	955		4	
21	12,850	22,605	245		1	
Apr.						
1	21,225	21,090	135		1	
11	20,850	20,715	135		1	
21	20,610	20,425	185		1	
May						
1	20,710	19,040	1,670		8	
11	19,390	19,295	95		—	
21	19,680	19,585	95		—	
June						
1	17,970	18,155		185		1
11	13,350	13,435		85		1
21	8,800	8,885		85		1
July						
1	9,810	9,895		85		1
11	10,900	10,975		75		1
21	11,970	12,055		85		1
Aug.						
1	13,280	13,255	25		—	
11	13,800	13,805		5		—
21	12,815	12,810	15		—	
Sept.						
1	11,850	11,935		85		1
11	10,870	11,055		185		2
21	9,920	10,045		125		1
Oct.						
1	9,080	8,765	315		3	
11	8,820	8,915		95		1
21	8,620	7,185	1,435		16	
Nov.						
1	4,320	4,005	315		7	
11	520	1,220		700		133
21	1,840	2,540		700		38
Dec.						
1	2,995	3,095		100		3
11	2,945	3,025		80		3
21	3,200	3,295		95		3
	457,200		−6,045	+3,670		

culation approaches the actual population trend to a very high degree. All high percentage deviations are at the population minimum, where the absolute deviations are of rather small importance.

6. CONCLUSIONS WITH REGARD TO THE SEASONAL POPULATION TREND OF THE HONEY-BEE, WITH SPECIAL REGARD TO AGE-DISTRIBUTION.

(A) Discussion of the premises

The figures given here are apparently the first dynamic analysis of any animal population (excepting that of man). It is therefore important to discuss the reliability of the premises on which the calculations were based.

(1) The fixed duration of every age of the honey-bee, determined for the egg, larva and pupa kept at a constant temperature of 35°C. The respective ages have been confirmed by so many observers that they cannot be doubted. The duration of nurse-, house- and field-stage in the adult worker-bee is based on the observations of Roesch (1925) and Morland (1930). Ten days is the rule for nurse-bees. The house-bees, which Morland and others call control-bees, may serve some days longer as nurse-bees, should the structure of the colony thus require. However, 10 days are quite usual as the average duration of this stage. The least certain data are those pertaining to the longevity of the field-bees. We have accepted here the generally assumed figure of 7 weeks total longevity of the adult worker. Actual observations and experiments by Morland (1930), Phillips (1928), and others show that the longevity of the field-bee rather varies, following the environmental conditions and the working intensity of the field-bee itself. In summer, life duration is probably less (1–2 weeks), whereas in spring and autumn it may extend over 2–4

weeks. The still higher longevity (150–200 days) in the winter is (probably causally) connected with the interruption of the brood-rearing cycle. This fluctuation in the life-duration of the field-bee has not been expressed in our calculations. The mistake involved is of no vital importance as no worker bees of any age participate in normal reproduction. Corrections which could be made only after sufficient empirical data have been collected—the latter lacking at present—would affect a reduction of the worker bees present in the summer. A hypothetical correction has been attempted for the Palestine colony during the short interruption of the brood-rearing cycle. The dynamic trend of the colony is not influenced by such corrections of the workers' life-duration.

(2) Successfully developing eggs do not show a considerable mortality under normal conditions. This premise is also based on the experience of very many experienced bee-keepers. While there exists reliable evidence that—occasionally at least—the queen may lay more eggs than can develop, it is a rule that any egg having started development or having transformed into a maggot practically never dies during its development. The same is true for nurse- and house-bees provided, of course, that no diseases or predators invade the hive. In the latter case, however, a colony would no longer be regarded as normal. The mortality of the field-bees, as far as it is independent of age, activity and environment, depends also on the amount of predators present and on the percentage of bees which fail to find their way back to the hive. Here again we know little about the field-bee.

(3) The fertility of the queen does not differ considerably from day to day, but changes only gradually. This assumption would seem to be in contradiction with some results of our calculation. However, our calculation yields practicable values only in its accumulation and no responsibility can be taken for the *daily* oviposition obtained by it. In nature, the daily transitions are smooth, as far as observed. In order to come closer to reality the daily oviposition data should be smoothed (as averages of 3–5 days). However, since we use in this analysis only the accumulations of 3 successive days no such smoothing seems to be necessary. Actually abrupt changes are observed and these are induced by (a) abrupt changes of environmental conditions (weather, etc.), (b) a major change of the oviposition place affected by the queen, or lack of space for oviposition, (c) conditions preceding swarming in a colony. Such abrupt changes will find an adequate and significant expression by our method of observation and calculation.

Our premises are, therefore, sufficiently sound to serve as a basis for our calculations and analysis.

(*B*) *The seasonal shift of the age structure*

Our analysis makes the study of the age-structure and its seasonal shift in a bee-hive possible for the first time. Fig. 9 gives a bimonthly survey for the single walled hive in Palestine. The identity of the age-structure with that of human populations is obvious. The pyramid represents a growing population, the bell a stagnating and the urn a decreasing population. The bee-population in January shows a very broad base and relatively few individuals in the medium ages. This anormality is smoothed in March, when the population is at its quickest rise. In May the population has grown more or less stable (in reality it has just begun to decline) and shows a practically equal distribution of all ages (bell). The decrease has passed its peak in July. Whereas the

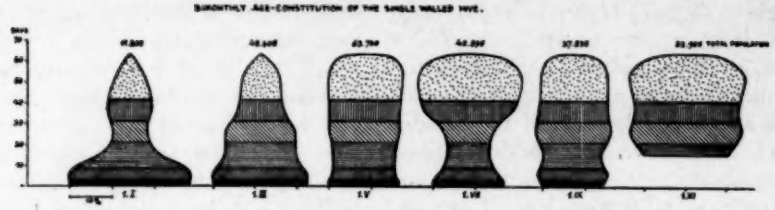

FIG. 9. AGE SHIFT IN A PALESTINE COLONY OF HONEY-BEES, ILLUSTRATED BY THE AGE CONSTITUTION OF THE SINGLE WALLED HIVE NEAR JERUSALEM

(Black: eggs. Diagonal lines to right: larvae. Horizontal lines: pupae. Diagonal lines to left: nurse-bees. Vertical lines: house-bees. Dotted: field-bees.)

TABLE 16

DATE	NO. OF INDIVIDUALS PER AGE-DAY						PERCENTAGE OF EVERY AGE*					
	Jan. 1	Mar. 1	May 1	June 1	Sept. 1	Nov. 1	Jan. 1	Mar. 1	May 1	June 1	Sept. 1	Nov. 1
Field-bees	111	475	1,090	910	585	464	7	9	18	25	16	28
House-bees	166	632	1,039	835	639	418	11	11	17	22	17	25
Nurse-bees	124	947	1,039	461	667	455	8	17	17	12	18	27
Pupae	208	942	892	396	542	334	14	17	16	10	14	20
Larvae	442	1,227	886	543	623	0	29	23	15	14	17	0
Eggs	438	1,232	998	623	567	0	29	23	17	17	15	0

* The computation of the age- per day percentage of the bee-population does not change the picture demonstrated by the drawings. It is therefore omitted here.

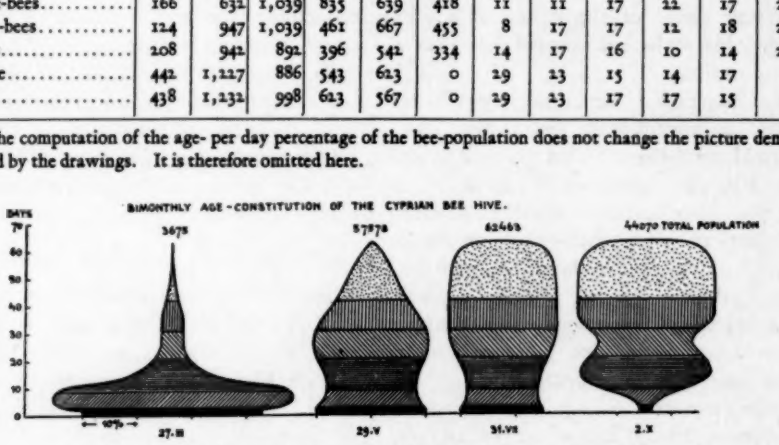

FIG. 10. AGE SHIFT IN A COLONY OF CYPRIAN BEES NEAR BALTIMORE

(For explanation see Fig. 9)

TABLE 17

DATE	NO. OF INDIVIDUALS PER AGE-DAY				PERCENTAGE OF EVERY AGE*			
	Mar. 27	May 29	July 31	Oct. 2	Mar. 27	May 29	July 31	Oct. 2
Field-bees	5	504	1,096	851	0.5	8	19	25
House-bees	45	1,062	1,198	844	4	17	20	25
Nurse-bees	42	1,323	1,053	563	4	21	19	17
Pupae	172	1,041	743	804	15	17	14	24
Larvae	496	1,057	720	236	44	17	13	7
Eggs	370	1,270	868	70	33	20	15	2

* See footnote to previous table.

older and medium ages show a typical urnal distribution, the number of eggs and larvae shows a very recent rise, which reaches its peak about the beginning of September. In November, any increase is interrupted and the older ages (pupae to field-bees) show an urnal distribution.

The absolute number of individuals per day-age and the percentages of every stage are compiled in Table 16.

The same conclusions are correct for the age distribution of the Cyprian bee (Colony 1926, Nolan 1928), which is compiled in Table 17 and in Fig. 10.

The transition from pyramid to bell and from bell to urn is also obvious here.

It is worthwhile to stress the similarity in the age structure of bee- and human populations, because of the very great difference in the methods of reproduction. While it is concentrated in one individual in the bees, it is more or less evenly distributed over all the middle-aged human individuals.

(C) *General conclusions*

The seasonal growth of bee-populations follows closely the logistic curve which the classical studies of Pearl (1926) have shown to be of value for animal populations in general. In Palestine as well as in moderate North America the period of actual population increase of the hive is identical (ranging from 100–126 days, or from 14–18 weeks). The seasonal shift in age-structure of the bee-population likewise follows the shift known from human populations, despite the very different type of reproduction.

There are a series of problems in bee-populations which have not been treated in this first analysis. The more important are the influence of swarming on the dynamic trend and the comparison of total annual bee-production in countries with a prolonged hibernal interruption of the brood-rearing cycle with those where this interruption is absent or reduced to a very short period. More material will have to be collected, before the analysis of these and other problems can be astempted.

It gives the writer a great deal of pleasure to acknowledge the help of Mr. A. Ben Nerya, Dr. S. Peller and Mrs. H. Rubel in the preparation of this paper.

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A CRITICAL REVIEW OF SOME RECENT WORK IN SEX DETERMINATION. I. FISHES

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IN MY book on sex intergrades (1931) I reviewed the facts then known about sex determination in fishes and came to the conclusion that in a general way conditions seemed to be very similar to those in the Amphibia, but that important data were still missing. During the past few years considerable work has been done in this group, which unfortunately, however, has not only not clarified the situation but made it even more obscure. In my opinion this is not the consequence of a special complication of the case, but rather of the complicated way of reasoning of the authors who interpreted the facts. We shall therefore review the facts and try to find out whether they do not after all fit into the general scheme derived from other groups of animals.

1. KOSSWIG'S WORK

Kosswig and his student Breider worked with *Xiphophorus*, some species of *Platypoecilus*, and some species of *Limia*. When they started their work it was definitely known that *Platypoecilus maculatus* follows the Abraxas type of sex determination XY = female, XX = male, as proven by sex-linked inheritance of dominant color genes (Gordon, Bellamy). It was further known that *Xiphophorus* shows complicated sex behavior of the type known in Amphibia: the males pass an early female stage, as do the so-called undifferentiated races of frogs, and, in addition, a considerable number of adult females of different ages may still be transformed into males, corresponding to the so-called adult hermaphrodites of frogs. Kosswig proceeded to cross these two genera, which is feasible. (In the following account we shall not use the nomenclature employed by the author, which is in part very tedious and cumbersome and in part derived from preconceived ideas of interpretation. We call the heterozygous sex simply XY, and the homozygous XX.) The following are the actual results. The X-chromosome of *P. maculatus* was in most cases marked by the dominant gene for red color, Dr. There is also a gene for black color, N, within the X-chromosome of *P. maculatus*.

First group

1. *helleri* ♀ × *mac.* ♂: ♀ ♀ and ♂♂ in the ratio typical for *Xiph.* = 75% ♂♂
2. *mac.* ♀ × F_1 ♂: ♀ ♀ and ♂♂ 1:1
3. F_1 ♀ × *mac.* ♂: almost only ♂♂
4. *mac.* × *helleri*: ♀ ♀ and ♂♂ 1:1

Second group with color markers

5. *helleri* ♀ × red *mac.* ♂: ♀ ♀ and ♂♂ red
6. F_1 ♀ × *helleri* ♂: red ♀ ♀ and ♂♂ (more ♀ ♀), gray ♀ ♀ and ♂♂ (more ♂♂)
7. RF_2 red ♀ × *helleri* ♂: gray ♀ ♀ and ♂♂, red ♀ (only one case)
8. Red. *mac.* ♀ × *helleri* ♂: red ♂♂, gray ♀ ♀
9. Red *mac.* ♀ × red F_1 ♂: ¼ red ♂♂, ¼ red ♀ ♀, ¼ gray ♀ ♀

From these crosses Kosswig draws very strange conclusions. Cross 3 does not contain a Y-chromosome from *Platypoecilus* and whereas no females (or only a few!) appear, Kosswig concludes that the female determiners in *Platypoecilus* must be contained in the Y-chromosome. The

X-chromosome of *Platypoecilus* is then supposed to contain no sex genes. This conclusion becomes possible only if it is assumed in addition that in *Xiphophorus* there are no sex chromosomes and no sex genes. *Xiphophorus* is assumed to be a hermaphrodite with no sex genes whatsoever, in which sex is determined phenotypically by external influences. But, of course, as the crosses of the second group prove, one of the *Xiphophorus* chromosomes is homologous to the X of *Platypoecilus*, giving a typical case of sex-linked inheritance of the Abraxas type (cross 8). Why does Kosswig then conclude that *Xiphophorus* has a phenotypic sex determination? First, because in RF_2 No. 6 red females appear also, which would not be possible if *Xiphophorus* females were XY; secondly, because such a red RF_2 female further backcrossed with a *Xiphophorus* male produces males and females without red; thirdly, because in further generations individuals may be produced containing both chromosomes from *Xiphophorus* supposed to be homologous to the Y of *Platypoecilus* (discerned by the absence of the markers); these are of both sexes.

There is a third unexpected conclusion: The male determiners in *Platypoecilus* are assumed to be scattered over the autosomes. The only reason for this assumption which one can find is the following: In F_2 of the cross *Xiphophorus* × *Platypoecilus* individuals are obtained with two *Xiphophorus* sex chromosomes (there are none such in Kosswig's terminology) and among these there are still more males than in pure *Xiphophorus*, namely, 90 per cent, which is laid to the action of autosomal male determiners derived from *Platypoecilus* and increased by recombination. Finally there is a fourth new assumption: If a *Xiphophorus* female and a *Platypoecilus* male are crossed, both sexes appear. But as it is assumed that *Xiphophorus* has no sex chromosomes but phenotypic sex determination and the *Platypoecilus* male is XX, it is concluded that this F_1 shows phenotypic sex determination, like the mother; therefore the anlage for phenotypic sex determination is inherited within the protoplasm. In the reciprocal cross, however, the protoplasm is derived from *Platypoecilus*, and in addition the Y-chromosome is at work and therefore normal genotypic sex determination takes place.

These are certainly astounding conclusions. Upon them Kosswig constructs additional general views regarding sex determination. We shall not report them here, not only because they contain too many contradictions but because they are of interest only if the foregoing conclusions can stand on critical examination. Let us first try to find out whether the results of the crosses, as far as they go, require any explanation different from the usual one. *Platypoecilus* has a perfectly normal sex determination of the Abraxas type, and there is therefore no *a priori* reason visible why the usual formulae should not be applied: FFMm = female FFMM = male with M within the X-chromosome, no sex gene in the Y, and F presumably in the autosomes, and the balance conditions FF>M<MM. *Xiphophorus*, on the other hand, behaves in regard to sex physiology exactly as the undifferentiated races of frogs, with transitory intersexuality of the males (a female phase) and adult transformation of a part of the females. Therefore the analysis of the frog case (mainly due to Witschi) ought first to be applied to this animal before new explanations are sought. There is *a priori* the possibility that male or female heterozygosis is present in *Xiphophorus*. The crosses with *Platypoecilus* ought to give information. Crosses 5 and

8 demonstrate female heterozygosis. A cross between both forms ought therefore to be parallel to a cross between differentiated and undifferentiated races of frogs, where, however, the male is the heterozygous sex.

The formulation which Witschi uses for differentiated and undifferentiated frog races, derived from our formulae for *Lymantria*, is:

Differentiated race: MMFf = ♂ MMFF = ♀
Undifferentiated race: MMFF$_1$ = ♂ MMFF = ♀

This means that M is situated within the autosomes and F within the X-chromosomes. In the males of the heterozygous sex the Y contains a relatively strong female gene F_1; in the differentiated races the Y either does not contain an F gene or only a very weak one, both cases being written f. Different races of different degrees of differentiation might then contain different grades of F from $F - F_1 \rightarrow F_2 \ldots .f$. Witschi's results further lead to the assumption that with increasing strength of the F_1-f genes in the Y-chromosome the strength of the F within the X-chromosome decreases, and vice versa. The full formulae then would be (assuming F_1 to be a little weaker than F; F_6 = f; str. = strong; w = weak; Au = autosome):

M(Au) M(Au) F_{str} (X) f or F_6 (Y) = differentiated ♂
M(Au) M(Au) F_{str} (X) F_{str} (X) = differentiated ♀
M(Au) M(Au) F_w (X) F_1 (Y) = undifferentiated ♂
M(Au) M(Au) F_w (X) F_w (X) = undifferentiated ♀

Applying these formulae to our case, where female heterozygosis obtains, we get:

F(Au) F(Au) M_{str} (X) m or M_6 (Y) = ♀ *Platypoecilus maculatus*
F(Au) F(Au) M_{str} (X) M_{str} (X) = ♂ *Platypoecilus maculatus*
F(Au) F(Au) M_w (X) M_1 (Y) = ♀ *Xiphophorus helleri*
F(Au) F(Au) M_w (X) M_w (X) = ♂ *Xiphophorus helleri*

These are, of course, the formulae applicable to all animals and plants with the additional assumption of a more even balance in the case of more hermaphroditic undifferentiated forms (a complete balance MM = FF would, of course, mean an hermaphrodite).

Let us now apply these old formulae to the crosses reported above

Cross 1. *helleri* ♀ × *maculatus* ♂
$M_w M_1 \times M_{str}M_{str}$
$\frac{1}{2} M_w M_{str}$ $\frac{1}{2} M_1 M_{str}$
♂♂ ♀♀ with very strong male tendency, stronger than in *Xiphophorus*, therefore much sex transformation

Cross 4: *maculatus* ♀ × *helleri* ♂
$M_{str}m \times M_w M_w$
$\frac{1}{2} M_w m$ $\frac{1}{2} M_w M_{str}$
♀♀ ♂♂

Cross 2: *maculatus* ♀ × F_1 ♀ from 1
$M_{str}m \times M_w M_{str}$
$\frac{1}{2} M_w m$ = ♀♀ $\frac{1}{2} M_w M_w$ = ♂♂
$\frac{1}{2} M_{str} m$ = ♀♀ $\frac{1}{2} M_w M_{str}$ = ♂♂

Cross 3: F_1 ♀ from 1 × *maculatus* ♂
$M_{str}M_1 \times M_{str}M_{str}$
$\frac{1}{2} M_{str}M_1$ $\frac{1}{2} M_{str}M_{str}$
♀♀* ♂♂

* With very strong male tendency (stronger than in *Xiphophorus*) like above.

The formulae then express the results if we do not insist upon the exact number of females in crosses 1 and 3, with mostly males. But as Breider has shown that lines of *Xiphophorus* may be selected with a genetically different amount of sex transformation, i.e. percentage of males above 50 per cent, it is of no use to insist upon differences in the number of non-transformed females, as these crosses were made with unselected stock, among which lines occurred with as much as 95 per cent of males.

We come now to the second set of crosses, in which the color genes for red pigment are involved. Here another fact must first be mentioned which seems to have impressed Kosswig considerably. He finds that in broods containing the genes for red pigment comparatively many females appear; if, however, genes for black pigmentation are present males are predominant. The genes for black pigment in addition produce in the crosses and backcrosses to *Platypoecilus* melanotic tumors and high lethality. Strangely enough, Kosswig concludes that the red pigment genes act like female sex genes because the number of females is increased, i.e. the number of females not undergoing transformation. Upon this assumption he bases a phylogenetic speculation on the origin of sex genes, which however need not be reviewed, as the basic interpretation is already impossible consisting, as it does, in mistaking an action which either diminishes the amount of sex transformation in an undifferentiated (i.e. more balanced) form (or an action which affects the vitality of the males), for an action affecting primary sex determination.

Returning now to the further crosses, we notice that Nos. 5 and 8 are just what is to be expected on the basis of the color gene being a sex-linked dominant.

Cross 6: (*helleri* ♀ × red *maculatus* ♂) ♀ × *helleri* ♂. (The letter *r* means the presence of the gene for red.)

$M^r_{str}M_1 \times M_w M_w$

$\frac{1}{2}$ $M_w M_1$ $\frac{1}{2}$ $M^r_{str}M_w$

gray ♀ ♀ red ♂♂

The actual result in a summary of all cases was, however:

♂♂ red	♀ ♀ red	♂♂ gray	♀ ♀ gray
55	105	152	57

This is the result which mainly led Kosswig to his interpretations. His argument is: If both reds and grays appear in both sexes, sex cannot be determined here by genetic means; it must be determined phenotypically (this in spite of the very typical numbers). Now the result for the grays corresponds to the normal sex ratio; therefore it is the *Xiphophorus* contribution which controls the type of sex determination, according to Kosswig. The preponderance of females among the red offspring show him once more, however, that the red gene acts like a female sex determiner.

Of course, Kosswig saw that these results look very much like a case of crossing-over between the gene for pigment and the sex gene. He dismisses this possibility, however, with a few remarks, being fascinated by his belief in phenotypic sex determination. But if we look at the facts impartially, the most obvious explanation is a complete (50 per cent) crossing-over between the pigment gene and the sex gene M, which would produce the four classes in equal numbers:

1. $M_w M_1$	2. $M^r_{str}M_w$	3. $M^r_1 M_w$	4. $M_{str}M_w$
gray ♀ ♀	red ♂♂	red ♀ ♀	gray ♂♂

The gray females, however, have the genetic constitution of *Xiphophorus*, which means considerable transformation into males and therefore excess of males as is well known. The red males, however, have the constitution which is known already from the F_1 results to impair male vitality and to favor larger numbers of females. No reason is visible, therefore, for introducing new and improbable explanations. (Kosswig seems to think that a backcross involving a male heterozygous for the pigment gene and giving the same result excludes an explanation by crossing-over; but as a matter of fact exactly the same result is to be expected if the foregoing argument is accepted.)

Cross 7: As is easily shown, there is the same expectation as in cross 6. Only one brood was obtained in which the expected minority of red males was missing; probably a chance result, which is as good an explanation as those offered by Kosswig, involving perfectly gratuitous male determiners in the autosomes.

Cross 9: red *Platypoecilus* ♀ × red F_1 ♂
$M^rm \times M^rM$

1. M^rM^r	2. M^rM	3. M^rm	4. Mm
red ♂♂	red ♂♂	red ♀♀	gray ♀♀

which was obtained

Thus it has been shown that Kosswig's crosses gave in all cases with sufficient numbers exactly what was to be expected on the basis of the old explanation if *Xiphophorus* corresponds genetically to the undifferentiated frog races. The only fact which has to be added is the action of the red pigment genes detrimental to the survival of males (there is, by the way, a corresponding action of black pigment on females). There is no cogent reason—as a matter of fact, no reason whatsoever—for introducing one or all of the theories of Kosswig.

We shall now report a further set of experiments by the same author in which another species of *Platypoecilus*, namely, *variatus*, is involved, which has males dotted with black. The males of this form pass through a transitory female stage and consequently behave like the undifferentiated frog races. Unfortunately, hardly any information is given regarding the crossing of this form with the two other species. But there are a number of crosses between *variatus* and *helleri* × *maculatus* hybrids, producing triple hybrids. Regarding simple F_1 we learn only that *maculatus* females × *variatus* males produce both sexes (see Bellamy's results below). In all the following triple crosses the X-chromosome of *maculatus* was marked by the red pigment gene.

Cross 10: (*helleri* ♀ × *maculatus* ♂) ♀ × *variatus* ♂. Red ♀♀ and ♂♂, gray ♀♀ and ♂♂, namely: 50 ♂♂ 23 ♀♀ red, 23 ♂♂ 24 ♀♀ gray

Cross 11: *variatus* ♀ × (*maculatus* ♀ × *helleri* ♂) ♂ 39 ♂♂ 1 ♀ red, 9 ♂♂ 33 ♀♀ gray

These results lead again to complicated formulations. Now the supposedly empty X-chromosome of *maculatus* seems to act in a male direction; therefore it may contain male determiners, which, however, act only if *variatus* genes are present. The few red females and gray males need another complex explanation. The red gene was to act, as we learned, as a female determiner; therefore, in the author's opinion, the red female is regarded as the result of a feminizing action of the red gene. Regarding the four gray males, however, the *deus ex machina* is a series of male determining autosomes from the *maculatus* parent, supposed to be present and to act in some individuals. From a cross which we shall report later the further conclusion is drawn that in *variatus* the male is the heterozygous sex.

It is very difficult to give a concrete explanation of these results, because most of the elementary facts are unknown: neither is it proven that *variatus* is XY = male (see Bellamy below) nor has an analysis of the crosses *variatus* × the two other species been made. Under the circumstances we draw attention only to the following points: If the *variatus* male is homogametic like the *maculatus* male, the crosses ought to give the same results as the corresponding crosses with *maculatus*, which is not the case. Therefore some special feature must have been brought in by the *variatus* parent. We find in Kosswig's paper that *variatus* belongs to the undifferentiated races, all males starting development as females; genetically this means that the M genes within the

X-chromosomes are relatively weak (assuming female heterozygosis as in *maculatus*). But there is another important fact: The red triple hybrids show a different type of red color; further, the time of appearance and spreading of red color is different from that in *maculatus* and *maculatus* × *helleri* hybrids. Finally, the red hybrids show very slow sexual differentiation and *frequently do not become fertile at all*. This shows that the simultaneous presence of the gene Dr and something from *variatus* causes peculiar disturbances. We shall see at once that Bellamy's work shows that *variatus* is either homozygous as a female or that some *variatus* females are genetic males in the female phase (with male homozygosis). No final interpretation of the foregoing results can be given before this point has been settled. But in any case the features just mentioned will probably account for the peculiarities of the crosses, without recurrence to the new formulations of genetic sex determination.

There follow now a series of such triple crosses with rather strange results.

Cross 12: *variatus* ♀ × red ♂ from cross 10
2 red ♂♂ 7 gray ♀♀

Cross 12a: *variatus* ♀ × red ♂ from 11
56 red ♂♂ 79 gray ♀♀

Cross 13: *variatus* ♀ × gray ♂ from cross 10
33 ♂♂ 34 ♀♀ gray

Cross 14: gray ♀ from 10 × gray brother
43♂♂ 48 ♀♀ gray

Cross 15: *helleri* ♀ × gray ♂ from 10
20 ♂♂ 10 ♀♀ gray

Cross 16: red *maculatus* ♀ with color genes RSp × red ♂ from 10 (color gene Dr)
9 red ♂♂ 17 ♀♀ RSp 3 ♀♀ red

Cross 17: red ♀ from 16 × *maculatus* ♂
♂♂ almost exclusively

Of these crosses, 13–15 are without special significance; 12, 12a, and 16, however, are remarkable because daughters are like mothers and sons like fathers (in 16 a few daughters are like the father). We shall see at once that Bellamy obtained the same result with *variatus* female × (*variatus* × *maculatus*) male and explained it simply by female homozygosity of *variatus*. The difficulties of such an explanation will be pointed out, pending further information. But cross 16, where the mother is *maculatus*, ought not to give such a result. Here again is a difficulty which cannot be solved before it is known whether *variatus* has female heterozygosis or male heterozygosis with protracted female phase in the genetic males. Judgment of these results must therefore be suspended.

We do not need to review the additional assumptions which Kosswig has to make to cover these cases. So far as can be judged from the very incomplete series of crosses, everything may be explained on the basis of ordinary genetic assumptions, applying the old formulae for female heterozygosis to all these forms, keeping in mind that we are dealing partly with undifferentiated races and similar special features.

Recently the same problem was attacked by Bellamy. Through his courtesy I had access to his MS, from which I am permitted to quote. The results he had thus far after crossing *variatus* and *maculatus* are:

1. *variatus* ♀ × *maculatus* ♂: only ♂♂
2. *maculatus* ♀ × F_1 ♂: ♀♀ and ♂♂ in equal numbers
3. *variatus* ♀ × F_1 ♂: ♀♀ and ♂♂ in equal numbers
4. *maculatus* ♀ × *variatus* ♂: many ♂♂, few ♀♀, 1 intersex

The *maculatus* X-chromosomes were marked with a color gene and in cross 2 about ⅓ of the individuals showed

its presence, and ½ did not. In cross 3 daughters were like the mother and sons like the father. Bellamy concludes from these preliminary facts that the *variatus* female is homozygous, and it is easy to work out the general results on this basis. But there are a number of difficulties and, in addition, the results are not in harmony with all of Kosswig's results. To mention only a few points: (1) The description of the race *variatus* shows it to be different from the form with which Kosswig worked. (2) Why are all individuals of cross 1 males? They might equally well be females, if the general formulation derived from *Lymantria* or *Drosophila* applies. (3) Why do Kosswig's crosses never yield only males? (4) In cross 2 why are there not produced equal numbers of wild type and colored individuals? Is there no alternative explanation? Thus far wherever unisexual crosses have been analyzed in these fishes and in Amphibia (see Aida below) it has turned out that one of the parents was a sex-reversal individual with the genetic formula of the original sex. As a late sex reversal was recorded in some of Kosswig's as well as Bellamy's material, it might be that in *variatus* genetic males in a protracted female phase (with a chance of late sex reversal) are found frequently and that Bellamy worked with such, which would give exactly the same results as were obtained, whereas Kosswig crossed both genetic females and males in a female phase. Further speculation on this point is not needed, as Bellamy has started experiments on a large scale which will furnish the information still lacking. Whether the scale will turn in favor of heterozygous females or males in a female phase, it seems certain that the explanation of the results will not require the complicated assumptions of Kosswig.

There is, finally, a set of experiments with another form of *Platypoecilus*, *P. xiphidium*.

1. Cross *xiphidium* ♀ × *maculatus* ♂, only ♂♂
2. Cross *xiphidium* ♀ × F1 ♂, red sons and gray daughters
3. Cross triple hybrid ♀ with RSp genes and X from *variatus* × *xiphidium*, red and gray ♀♀ and ♂♂

From crosses 1 and 2 the author concludes that in *Platypoecilus xiphidium* the male sex is again heterozygous. To make this work it must be further assumed that the male genes from *maculatus* (supposed to be in many of the autosomes) are dominant. But cross 2 requires the assumption, as was the case before, that unknown genes in the X of *maculatus* act in this peculiar combination like male determiners. We notice at once that these crosses correspond exactly to Bellamy's crosses 1 and 2. The comment is therefore the same. There will be much more work required to elucidate these cases; until this is done it will be of no use to introduce complicated new assumptions, to claim that they have been proven when this is not the case, and to erect even phylogenetic speculations upon obscure facts.

Kosswig's student, Breider, has made a similar series of experiments with another genus, *Limia*. *L. nigrofasciata* is an undifferentiated race, the males having an early female phase, but finally with a normal sex ratio. *L. caudofasciata*: The conditions are not perfectly clear, though it seems that this also belongs to the undifferentiated forms, and *L. vittata* the same. The two latter forms behave somewhat like *Xiphophorus* in so far as sex changes seem to occur easily. For *vittata* a great variation in the sex ratio and typical juvenile hermaphroditism have

been shown recently. The results of all his crosses are summarized in Table 1, in which numbers are mentioned only when they depart from a 1:1 ratio.

Let us look at these crosses without prejudice. We find three types: (1) a majority of combinations with a 1:1 ratio of the sexes; (2) a considerable siderable variation of the sex ratio, though the details are by no means clear and no exact data are given concerning later sex transformations. All these crosses point to very simple conditions; namely, female or male heterozygosis in all three forms, decision depending upon sex-linked genes and an additional hereditary inclination

TABLE 1

ni = nigrofasciata; cau = caudofasciata; vitt = vittata

NO.	CROSS	♀	♂	REMARKS
1	ni × cau	168	—	Up to 4 months of age.
2	cau × ni	50%	50%	
3	(ni × cau) × cau	219	50	
4	(cau × ni) × cau	176	60	
5	(cau × ni)^2	50%	50%	
6	(ni × cau) × ni	50%	50%	
7	(cau × ni) × ni	50%	50%	
8	ni × (cau × ni)	50%	50%	
9	(ni × cau) (cau × ni)	50%	50%	
10	cau × (cau × ni)	50%	50%	
11	vitt × cau	23	14	3 undiff.
12	cau × vitt	26	27	15 undiff.
13	vitt × (vitt × cau)	213	29	
14	vitt × (cau × vitt)	198	41	
15	(cau × vitt) × vitt	267	108	
16	(vitt × cau) × cau	50%	50%	
17	cau × (vitt × cau)	50%	50%	
18	cau × (cau × vitt)	50%	50%	
19	(vitt × cau)^2	50%	50%	
20	(cau × vitt)^2	50%	50%	
21	(cau × vitt) (vitt × cau)	50%	50%	
22	ni × vitt	50%	50%	3–4 months old ♂ still in ♀ phase!
23	vit × ni	50%	50%	
24	(ni × vitt) × vitt	81	12	
25	vitt × (ni × vitt)	21	2	
26	ni × (ni × vitt)	55	9	
27	(vitt × ni) × ni	50%	50%	

number with preponderance of females; (3) one cross, number 1, with only females. If we look at the second group, all but one (26) of the combinations are backcrosses with double doses either of cau or of vitt. These two forms, however, are described as undifferentiated races with inclination to the formation of the males after a female phase and con- towards more or less late male differentiation after a female stage in *caudofasciata* and *vittata*. This is enhanced in a hybrid if two sets of chromosomes from undifferentiated forms are present. All this is within the limits of known occurrence among such fishes. There remains only one cross, No. 1, with nothing but females. It is worth while to compare this

with cross 22, which is ni × vitt instead of ni × cau. There is *a priori* no reason why these two should not give the same result, whatever the interpretation. For cross 22 we find it noted that sexual differentiation is very slow, that some individuals do not become fertile after a year; in females 3–4 month-old ovaries were found with additional male tissue. All this means that the genetic males have a protracted female phase. I should therefore conclude that the exactly parallel cross 1 is of the same type and that the late transformation of genetic males has been overlooked because not expected. If this is correct, and all the facts point in this direction, the whole experiment is indeed a very simple one.

Breider, however, is of a different opinion. Fascinated by Kosswig's theories he takes cross 1 as the starting point and concludes that the species *nigrofasciata* has male heterozygosis, the two others, however, phenotypic sex determination. To explain all the other crosses he needs the whole arsenal of Kosswig's assumptions, dominance of one type of sex determination over the other, empty X-chromosomes, which are in some cases not empty, multiple autosomal genes, etc. As facts stand at present, however, there is no need for all these assumptions. We conclude: The experiments of Kosswig and Breider fall in line with what we know from other organisms, especially frogs. The few special features may well be explained on ordinary genetic lines, like crossover, lethal effects, etc. For an exhaustive analysis the material is far from sufficient. There is thus far no ground for Kosswig's interpretations and the whole body of his speculations based upon these (not reviewed here). There is especially no base found in the facts that such things exist as phenotypic sex determination, male and female heterozygosis within the same genus, empty X-chromosomes, female determiners in the Y, multiple autosomal male determiners, etc.

2. WINGE'S WORK

During the past few years Winge has published a number of papers on his crosses with the genus *Lebistes*. In this case also a theory of sex determination has been derived which he thinks is different from the older ones and which differs also from Kosswig's conclusions.

The actual facts in question are the following:

(1) In *Lebistes* the male is the heterozygous sex, as proved by sex-linked inheritance.

(2) The Y-chromosome contains many genes (for color), some of which cross over to the X, while others do not.

(3) Ordinarily the females do not show any effect of the color genes, which act in a sex-controlled way.

(4) *Lebistes* is one of the forms in which transformation of females into males occurs, parallel to the well-known cases in frogs. The details of this phenomenon are not sufficiently known to make a complete comparison with the frog case.

(5) In a cross with X- and Y-chromosomes marked by color genes three exceptional males appeared showing the colors required by the presence of both X-, i.e. XX males or, as we called them in *Lymantria*, males by transformation. This is not so very surprising, as such transformations are known to occur here, but in this case, fortunately, the markers prove the XX constitution. (In frogs and *Lymantria* the same proof was furnished by the later sex ratios only.)

(6) These three XX males crossed to unrelated females produced only females, which then is a repetition of Crew's experiment with frogs and of similar experiments with *Lymantria*.

(7) Some of these daughters were backcrossed to the father and again only females were obtained.

(8) Granddaughters again backcrossed to the grandfather gave 30 females and one XX male.

(9) The same granddaughters mated with their XX sons gave ½ females ½ XX males.

(10) Continued breeding from such females and XX males gave extremely different results: sometimes half females half males, sometimes only females, sometimes many females and few males, and sometimes also more males than females.

(11) There seemed to be an influence of the seasons, as one and the same female produced both sexes in summer and only females in winter.

(12) Crossing these XX males with unselected females produced only female offspring.

From this first set of facts Winge draws the following conclusions: The X-chromosomes contain a rather potent female determiner, and the Y-chromosome a strong male determiner. But in addition the autosomes contain many female and male determiners of different value. Ordinarily the total value of these latter ones is balanced and they do not exert any influence upon sex determination. But if, e.g. by selection their values are shifted to the female or male side, they may accumulate so as to override the action of the sex genes within the sex chromosomes. This is exactly what Winge believes to have happened. His males by transformation (XX) he believes—of course, without any proof—to have arisen by the fortuitous accumulation of male autosomal genes of high male value. The two backcrosses which he made with these he believes to have been a selection experiment with these autosomes. When in the following backcross an XX male appeared again, he thought that this selection was again successful. And when in the next generation he obtained ½ XX males he claimed that he had selected a very strong male autosome which henceforth would act like a sex chromosome. In his opinion he has proven the transformation of an autosome into a sex chromosome and of the sex chromosomes into autosomes!

This is certainly a most interesting claim which calls for a closer examination of its foundations. Winge does not have a very good opinion of the older views regarding sex determination (the FM-balance formulae). In his opinion "the formulas may perhaps be justified in a text-book for students where it is often necessary to present the phenomena in a simplified way, but they cannot be recognized as being perfectly valid scientifically." Thus it may be of interest to find out, first, where the alleged superiority of his formulation comes in.

(1) Winge needs the major F-genes in the X-chromosomes just like the older authors.

(2) He assumes a major M-gene in the Y-chromosome, just as Goldschmidt did for a long time in *Lymantria* (here F, of course) until he could prove experimentally that the action in question was protoplasmic. As a matter of fact, there is not a single experimental fact in Winge's papers which would prove the presence of a sex gene in the Y-chromosome.

(3) Winge assumes the presence of many female and male (+ and −) genes in the autosomes, which, however, are without effect under normal circumstances. In ordinary genetic terminology this would mean that many autosomal genes may act as modifying genes on either sex, either if mutated or if recombined in a definite way. One such autosomal modifier was also found a long time ago in *Lymantria* and analyzed genetically (the gene T).

At closer inspection, then, the only difference between the old formulation and Winge's view is that the older authors claimed the presence of autosomal modifiers only when they were actually found, whereas Winge introduces them in large numbers *a priori*. This would certainly not do for a textbook; but is it really justified scientifically? The answer will be given by considering whether the new facts found by Winge, namely, the reappearing of XX males after a few backcrosses to normal females and their daughters and granddaughters, are actually outside the reach of the old formulation.

The explanation applied formerly to cases of sex reversal of the present type (e.g. Crew's case, *Lymantria*) was that in the female MMFF, where F > M either a weak F or a strong M was introduced (either by presence in the stock, or by mutation, or by appropriate crosses, as

in *Lymantria*). The formula for such a male by transformation would be either $M_{str}MFF$ or $M_{str}M_{str}FF$ or MMF_wF or MMF_wF_w. As the cross with an unrelated female actually gives only females, it follows that the XX must be homozygous for the change in question, i.e. either $M_{str}M_{str}$ or F_wF_w. Both of these give the same results, of course, and we therefore apply only the one assumption, MMF_wF_w, which allows us to dispense with autosomal MM in writing the formulae:

1. $FF \times F_wF_w = FF_w$ = all ♀♀
2. $FF_w \times F_wF_w = FF_w, F_wF_w$ ½ ♀♀ ½ XX ♂♂

The actual result was that No. 2 gave only females the first time, one male the second time, and half males only the third time. This is all important to Winge because he wants to prove that in these three generations he has selected the autosomal male genes. But then he continues breeding within this line and he gets, as reported, sometimes only females, sometimes a few males, and sometimes both sexes, though the formulae must remain the same. I cannot understand why this is in any way different from the result of the backcrosses and why two perfectly identical results must have a thoroughly different explanation, one by selection of assumed autosomal genes, the other by phenotypic influences. In my opinion the results agree completely with the old formulation as applied above. To which, however, it must be added that the F/M balance in the XX males is so close to even that small influences, e.g. seasons or modifying genes or unknown phenotypic influences, may shift the balance to one or the other side. (I should expect intersexes also in these crosses; they are not mentioned, but probably no dissections were made.) There is, then, nothing in the facts recorded by Winge which calls for theoretical assumptions beyond the well-established old ones. And it must be emphasized most strongly that Winge's claim of having transformed a sex chromosome into an autosome has no foundation in the facts reported by himself. Moreover, his experiments give no information whatsoever upon eventually existing autosomal modifiers, and, further, they give no information whatsoever upon eventual sex genes in the Y-chromosome. They have changed nothing as yet in the aspect of the problem of sex determination.

There is another set of facts in Winge's publication. In one cross with marked chromosomes he found seven females showing a Y-chromosome character. (The accompanying picture shows this character to be very different, however, from the same in males! In addition, it is very exceptional in *Lebistes* that females show color at all.) It is therefore assumed that these are females XY, i.e. females by transformation (parallel to males by transformation in *Lymantria*). An explanation by crossing-over of the color gene ma from the Y to an X is held to be impossible because crossover of this gene is said never to occur. (But Winge reports later that Mr. Eloff, who received his material from him, claims to have found such a crossover, with which Winge disagrees. There usually is, of course, no crossover in *Drosophila* males, but nevertheless it occurs.) The further crosses obtained with these XY females give the expected results, including the segregation of ¼ YY males, which are perfectly viable. From this he concludes again that here female determining autosomes have been selected and, in addition, that the Y contains the male determiners.

Leaving aside these speculations, there

is proof afforded that the exceptional females were XY females, namely, by ascertaining that the YY males actually exist.

Winge reports one cross of the presumably YY male with a normal female giving only 42 males, which indeed proves the point. These facts, of course, have nothing to do with the mode of sex determination. If it is proven that the Ma females were actually XY, the same reasoning would apply to these females by transformation as before to the males by transformation, both being paralleled by the *Lymantria* cases. We shall return to this below.

3. AIDA'S WORK

Aida has obtained in *Aplocheilus* what amounts to practically identical results with those of Winge, but he offers a larger bulk of experiments. Here, also, the male is the heterozygous sex and the X- and Y-chromosomes are marked by color genes. He also finds exceptional males and proves by their female offspring that they are XX males by transformation. Parallel to Winge's results, he further states that such males sometimes produce only females, sometimes a few additional males, sometimes females and males, and sometimes even more males. In reviewing Winge's work we have already seen that a proper evaluation of these facts disposes of all his theoretical claims. Aida has made a special study of this point which completely corroborates my criticism of Winge's claims (corroborates, because my criticism was written a long time before Aida's paper appeared).

By crossing individual sex-reversal males to different females in many combinations, Aida finds the following in regard to the appearance of males in smaller or larger numbers:

(1) There is an individual difference in regard to different sex-reversal males, some producing more, some less, male offspring. (2) There is an influence upon the result of individual females of the same line, some tending to produce more or less male offspring. (3) Wild type females always give exclusively females with the XX males, but for one occasional male. (4) There is a decisive influence of temperature upon the production of males in these combinations. (5) By selecting lines with relatively many males, combinations with more males than females may be obtained.

Without further discussion these facts show at once that Winge's claim of transforming autosomes into X-chromosomes is without foundation.

Aida found also in his form the sex-reversal XY female. His analysis led to the same results as Winge's: XY × XY = XX XY XY YY = 3 males: 1 female and the YY male viable; these produce only males with normal females, and these results, identical with those of Winge, are in this case thoroughly established by many crosses and combinations.

Aida gives a theoretical explanation of his results, which he believes to be a new theory. In fact it is only a restatement of the explanation derived from the *Lymantria* and frog work in a little different language. He thinks that the autosomes contain genes for the production of primary sexual characters. The action of these must be stimulated by "stimulating" genes in the sex chromosomes, some of which stimulate the male, others the female, sex characters. (This is, up to this point, the old view of Correns, which Aida overlooked.) Now Aida supposes that these sex-exciting genes (the realizer in Correns's theory) act by their quantity, i.e. there is a threshold and a quantity above this "excites" male, below it, female differentiation, as it may be, and of course the X-chromosome mechanism provides these two quantities.

In the sex-reversal fishes the potency or quantity of this substance fluctuates for some unknown reason and therefore goes beyond the threshold, more or less, dependent upon external and internal influences. This, of course, makes all the results possible. Aida proceeds then to apply the same formulation to the *Lymantria* work and finds that he can explain this in the same way. But he has not realized why, namely, because his formulation is in essence, though not in words, identical with mine. What I call F-genes (male heterozygosis) he calls exciters; what I call different quantities of F (one F, two F) balanced against the same quantity of M he calls different quantities of the exciter working upon the always equal genes of sexual differentiation. What I call epistatic minimum for the balance F/M he calls the threshold value for the male or female exciting action of the exciters. What I call weak and strong M he calls fluctuating quantities of the exciter. Thus of course, he must also get the same results, though in different language. Aida's explanation is then essentially the same as the one which we derived from the *Lymantria* formulations, though couched in different language. And the facts found by Aida prove indeed that this interpretation is correct, whatever language we are using, and that Winge's theories are without foundation and unnecessary.

Thus we see that all the interesting work done in this group of fishes fits, as far as it goes, into the theory of sex derived from the *Lymantria* work, without a single additional hypothesis being required if the old formulation is applied logically.

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MORPHOGENESIS OF THE SHOULDER ARCHITECTURE

PART VI. THERIAN MAMMALIA

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IN SO FAR as concerns the architecture of the appendages, the step from living reptiles to therian mammals is a great one; and in most respects the latter differ almost as widely from prototherians. In fact monotremes are more properly comparable with reptiles than with therian mammals, and they will receive no consideration in the present contribution. Accordingly, in this paper the term Mammalia refers to Theria. The chief reason for the dissimilarity in the pectoral appendages of these groups is attributable to the difference in the manner in which the limbs are used. In the prone position of the reptilian body, the largely horizontal position of the humerus, with divergent elbows, requires a very different muscular arrangement and bony scaffolding from that suited to the needs of mammals. The typically reptilian architecture of the shoulder is unsuited to speedy or long sustained action. In order to change to the mammalian plan it was necessary for reptiles to bring the elbow beneath the body, involving an alteration of the shoulder joint and its controlling musculature, which Reptilia found so difficult of accomplishment that only a single group (the Theriodontia) ever succeeded in its accomplishment. The resulting improvement in function doubtless played no inconsiderable part in the development of the class Mammalia. The result, in improved form, is that mammals may operate the limbs for purposes of locomotion in a single plane, for extended periods, with the expenditure of a minimum of energy.

SKELETON

In mammals the only surviving part of the membranous girdle is the clavicle. This no longer occurs partially parallel with the border of the scapular girdle, as is frequently the case in reptiles, but is attached to the acromion at one end. It is the first bone in the body to ossify, and this takes place from two primary centers, marking the attachments of mm. cleidomastoideus and trapezius. From these points chondrification and subsequent ossification spread in both directions. A secondary center appears at the sternal end much later (at 20 years in man). The fact that the clavicle arises from both membrane and cartilage has been interpreted (Parker, Howes, *et al.*) as indicating that it is a compound bone, involving arguments which it seems unnecessary to repeat here. At the scapular end of the calvicle is often a meniscus-like cartilage, and at the sternal end, two others. That one of the latter represents the procoracoid, as was at one time claimed, is extremely unlikely. Rather should both be considered as belonging with the sternum. Except for the irregularity in its embryology, there appears to be no evidence in comparative anatomy to indicate that the clavicle is other than a single element of the mem-

branous girdle. In two respects it is rather unique; it is a radius rod rather than an anchorage, and it is the only "long" bone of membranous derivation. Perhaps these facts account for the noteworthy features of its development.

In its rôle of radius rod the clavicle is useful to mammals which employ the manus as a tool. Usually it is stated that clavicles are absent in those mammals which use the forelimbs for support only. In a way this is so, but no rule of this sort can be given. Rather is it a fact that the clavicle furnishes anchorage to a part of the accessory field of musculature and a part of the deltoid. When these two fields fuse in part, reflecting the need for a cephalohumeral muscle, the clavicle tends to disappear. In addition to this stimulus there doubtless are others of a mechanical nature, associated with the function of the clavicle as a strut.

Well developed clavicles are present in all the Marsupialia except the bandicoots (Peramelidae), in Insectivora except *Potomogale*, in Dermoptera, Chiroptera, Primates, and Tubulidentata. The Edentata are variable; sloths, armadillos, and *Cyclopes* have clavicles, while the rest of the anteaters, and the Manidae, lack them. The Rodentia are extremely variable; some forms have strong clavicles, others lack them, and in still others they are vestigeal (as in Leporidae) or ligamentous (*Castor*). In terrestrial Carnivora clavicles are rudimentary, while Pinnipedia, Cetacea, Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla are supposed to be entirely aclaviculate. When present the clavicle is variously curved, the noteworthy exception being in the bat *Diclidurus*, in which this bone is trifid (Miller, 1907). It is attached to the sternum medially and the acromion laterally. When the clavicle is absent or vestigeal a raphe is supposed to take its place; but this raphe marks the point of junction of the accessory with the deltoid fields, and may not concern the clavicle.

Scapular girdle

I have already discussed the scapular girdle in previous parts of the present contribution. Probably all that need be offered here is a recapitulation of my beliefs. The scapula proper originally provided attachment for the suprazonal group of muscles. As the cleithrum disappeared the homologue of the teres minor and most of the trapezius settled upon the scapula, and then a part of the deltoid shifted to it from the clavicle. Later still a part of the latissimus settled upon it, to become the teres major, and origins of the supra- and infraspinati migrated to it from the procoracoid. With the exception of the secondary change of the two muscles last mentioned, then, the scapula is concerned with dorsal muscles only.

The coracoid provided an area for the origin of the coracobrachial group of muscles and for the shorter head of the biceps as the latter developed from the brachialis. As the angle of the brachium altered, in the change from the reptilian to the typical mammalian position of the limb, the muscle origins contracted and the bone retreated, to adopt the form typical of the mammalian coracoid process. It is interesting to note, however, that in early developmental stages of Marsupialia, the coracoid cartilage reaches the sternum, as reported by Broom in a series of papers. It is useless to speculate on the significance of this item, however, until we know what embryonic muscles are concerned. At least in adult members of the class, there arises from the coracoid process all the mammalian representatives of those muscles which in lower tetrapods originated

from the broader coracoid, or posterior coracoid, and the two must be considered as homologous.

The primitive procoracoid provided an area for the origin of the group of muscles that I have termed coracohumeral. The origin of a part of this musculature shifted to a firmer anchorage on the sternum, to become the pectoralis major, and that of the remainder migrated to the scapula, to become the supra- and infraspinati. Hence, the procoracoid musculature left the procoracoid completely, and the bone retreated toward the glenoid. But meanwhile the origin of one division of the biceps brachii may have become associated with the posterior procoracoid, so the bone may not have disappeared entirely. If the procoracoid is represented in therian mammals it is by the supraglenoid tubercle, indubitably, from which arises caput longum biceps brachii. This detail having been determined, it is, in my opinion, fruitless to argue further as to the actual presence or absence of the bone. The fact that this tubercle usually develops from a separate center of ossification is without phylogenetic significance, for such centers merely reflect local stresses.

To a greater degree perhaps than any other bone in the body, the conformation of the scapula is influenced solely by muscular action. In this, some muscles are particularly influential, others play a minor rôle, and in some cases the influences at work are complex.

The scapula is quite variable in form, in conformation of the glenovertebral and coracovertebral angles, position and shape of the spine and acromion, and in the coracoid process. Chiefly influential in moulding the glenovertebral angle is the caudal part of the serratus anterior, and to a much lesser degree, the teres major, while infraspinatus, subscapularis, and teres minor appear to have every little to do with the conformation of the angle. The caudal part of the serratus anterior is a depressor (ventral rotator) of the scapula and is used in protraction of the arm. Increase in the effectiveness of this action is correlated with a falciform shape of this angle. Conformation of the coracovertebral angle of the scapula is determined chiefly by the levator anguli scapuli, to a minor degree by the subscapularis, and probably to a negligible extent by the supraspinatus. The width of the scapula appears to depend upon a considerable number of factors, and not solely, or even chiefly, upon the width of the supra- and infraspinatus muscles, for these may be much narrower than their fossae. Of more importance is probably the subscapularis, and still more so, the optimum angle of leverage of the two vertebral angles.

The spine of the scapula is extremely variable. Undoubtedly its most important influence is supplied by the trapezius, for where that muscle is greatly reduced or absent there is no spine of any consequence; but the deltoid also has a considerable influence. Both muscles, and the levator scapulae ventralis when present, affect the acromion, which may be bifid. Usually the spine is attached to the scapula along a line extending from near the vertebral border to the spinous notch, at the neck of the scapula. Occasionally (in such dissimilar genera as *Lagostomus*, *Loncheres*, *Georychus*, *Ochotona*) this notch is located midway of the scapula blade, so that the attachment of the spine to the scapula is very short.

The relative width of the supra- and infraspinous fossae is quite variable, even in the same order (about 1:3 in man to 1:1 in gibbons). Moles are perhaps the most bizarre in this respect, the infraspinatus fossa being a minute groove in

Uropsilus, while *Talpa* and *Mogera* lack an infraspinatus muscle, so their infraspinous fossa is really a teres fossa (Campbell, MS). In moles and most shrews both fossae are so narrow that the whole scapula is markedly rodlike; but in others of this order (as *Erinaceus*, *Solenodon*, *Tupaia*) it is of the usual mammalian shape. It is also narrow in ungulates. Eared seals have a very large supraspinous space, and cetaceans one of negligible size. Notably in some bears and edentates the infraspinous space is divided into an infraspinous and a teres fossa by a ridge between, attributable to the action of the long triceps, or that and the teres minor together.

In some mammals (Cetacea, some ungulates) the vertebral border of the scapula tends to remain distinct, first as a cartilaginous and in later life often as a calcified margin of variable width. To this is attached the suprazonal musculature, and it is of sufficient phylogenetic importance to merit recognition as a separate element of the scapula. A suprascapular notch, for the passage of n. suprascapularis, usually is absent, but may occur (many primates, *Choloepus*), or even be present as a foramen completely surrounded by bone (Bradypodidae, Myrmecophagidae). A well defined coracoid process appears to depend chiefly upon m. coracobrachialis, and to a lesser extent upon caput breve biceps brachii. At least when the former muscle is absent the process is virtually undefined. The mammalian glenoid cavity needs little description, and yet it embodies one of the most significant changes that took place in reptilian-mammalian phylogeny. This altered the direction in which the glenoid faced, from caudolateral to ventral—a simple change but one which very few reptiles were able to acomplish. This, and a corresponding change in the head of the humerus, allowed the plane of the thoracic limb to be held at a right angle to the ground, so that the body could be supported with the expenditure of a minimum of effort.

Ossification of the mammalian scappula varies between different groups. In some of the smaller and the weaker sorts the centers appear to number no more than three—body and spine together, coracoid, and acromion, possibly with a separate center for the glenoid. Man is said to have as many as nine centers. At least the one for the body may be considered as primary, but it is impossible to determine which of the remainder have phylogenetic weight, for centers can disappear with the reduction of stress, or new ones appear as muscular stresses increase. The supraglenoid tubercle may fall in the latter category.

The change in the glenoid from the reptilian to the typically mammalian position was made completely possible only by the extreme reduction of the coracoids. A change in the articular surface of the humeral head was, of course, a requisite, but this doubtless was very easily accomplished. With this exception there was little necessary alteration in the free portion of the appendage. This part of the reptile limb can be placed in the mammalian position, but the ulnar border of the manus will then be presented to the ground, and pronation of about 90 degrees, through the forearm bones, will be required in order to present the palm to the ground.

So no twisting of the humerus appears to have taken place in the change from reptile to mammal. In the very earliest amphibian stage there was such a twisting, as explained by Romer (1931). This accounts, at least largely, for the tetrahedral form of the bone in early tetrapods, but thereafter there was only a reshaping,

an alteration in the presentation of the articular surface, and a slight migration of the processes to conform to the change in the angles of leverage of the muscles following the bringing of the limb beneath the body. The appearance of twisting is given by the last vestige of the originally tetrahedral shape, and by the facts that m. brachialis and n. radialis spiral about the shaft.

NERVES

With the exception of primates, accurate and complete descriptions of the brachial plexus of very few mammals indeed are to be found in the literature. I have investigated this detail in a number of orders myself, but I do not wish to delay publication of the present contribution until all orders can be dissected. On the whole, the pectoral limb of the more generalized primates, and of man, is of fairly "primitive" organization, and as the human brachial plexus is in basic respects an almost exact counterpart of that of the opossum (*Didelphis*) and of rodents, it is considered that it is entirely satisfactory as an example of conditions in Mammalia.

The mammalian brachial plexus may be said usually to have its axis through sp. n. 7 (n. 10 in *Bradypus*). This contrasts with conditions in other classes, in which the axis is through C. 3 in *Rana*, C. 4 in *Necturus*, C. 5 in *Chamaeleo*, presumably with intermediate conditions in other reptiles progressing to C. 8 in *Iguana* and C. 9 in *Crocodilus*. Accordingly it is seen that there has been a caudal migration of the brachial plexus, less pronounced in some living reptiles than in mammals and more pronounced in others.

The general plexus plan in mammals conforms to that characteristic of vertebrates, comprising three trunks, the first and third of these being joined by an additional nerve each, so that typically five nerves contribute to the plexus. Usually these are the nerves from C. 5 to T. 1 inclusive. Occasionally, in a prefixed plexus, C. 4 may be involved (man), or regularly T. 2 (n. 10) *Macaca*, although in *Bradypus* it extends to n. 12. Or in mammals in which there is a restriction in the diversification of movements, the plexus may be almost (*Canis*) or entirely (*Bos*, *Galeopithecus*) confined to four nerves. In *Equus* (Sisson) there are three main nerves (C. 7–T. 1), with very small contributions from C. 6 and T. 2. In this case the plexus is very definitely postfixed. Partly on this sort of evidence but chiefly because of rather complicated neurological considerations, it is indicated that no greater number of myomeres contributed to the formation of a mammalian limb innervated by six nerves, than to that of an amphibian innervated by two, but that a multiplicity of contributing nerves is a reflection of habitual complication of the movements of the member. Thus a restricted plexus, accompanying restriction of diversity of movement, is a primitive character in *Necturus* and a specialized characteristic of some mammals.

It is well nigh impossible to stress too strongly the importance of the fact that the limb plexus is divisible into dorsal nerve elements, innervating the extensor groups of muscles, and ventral elements to the flexor groups. Separation of the plexus as a whole into these two components does not take place until the level of the cords, although every one of the high branches belongs with one or the other, and each group has its cell bodies located in predictable parts of the spinal gray matter.

In Mammalia there are typically a lateral and a medial cord carrying ventral fibers, and between them a posterior cord formed of dorsal fibers. In cold-blooded

vertebrates this statement hardly applies, not because of fundamental differences in the plexus, but for the reason that splitting occurs in a slightly different manner. Occasionally, as in some rodents and Cetacea, the three-trunk plan is obscured and a two-trunk plan results. In all vertebrates there is a slight tendency for the ventral components of the plexus to occupy more linear territory than the dorsal part.

Often, in analyzing the plexus, it is impossible to determine precisely which nerves contribute to a particular branch. In such cases an approximation is all that is possible. In the following pages conditions in the uniquely post-fixed plexus of *Bradypus* are not included.

N. accessorius spinalis arises from the upper cervical part of the cord as far caudally in some mammals (*Equus*) as the seventh, or only as far as the first cervical segment (*Phocaena*, Hepburn and Waterston). Some of the fibers of accessory relationship, however, adopt a short cut and emerge with spinal nerves C. 2 to C. 3 or 4, and an increasing tendency for the accessory fibers to follow a cervical pathway may be considered to be a progressive tendency in mammals, culminating in some individuals of the long-necked ungulates (giraffe, camel, llama), in which all accessory fibers follow this pathway and the ramus externus n. accessorii is no longer present. These fibers innervate mm. sternocleidomastoideus and trapezius of the branchiomeric group of muscles.

N. omohyoideus is the only "hypobranchial" or ventral nerve of the neck that concerns us. It is one of the lower branches of the ansa hypoglossi, single or double; to the two bellies of m. omohyoideus.

Dorsal (extensor) division

Suprazonal group

Nn. thoracales posteriores consist of three closely-related entities that may well be considered as three subgroups of the same nerve element. The basic one of these is n. thoracalis longus, to m. serratus longus. In man, in which the muscle is not continuous with m. levator scapulae, the nerves to the latter are considered as separated from the long thoracic nerve, but in those mammals in which the two muscles are continuous, innervation may be also, from C. 2 to 7 or 8, contributing in addition to m. levator scapulae ventralis when that is present. The third part of the posterior thoracic nerve element is n. dorsalis scapulae, to m. rhomboideus. When this muscle is confined to the lower nuchal and thoracic regions the innervation usually is derived from C. 5; in cases in which the rhomboid is more extensive an additional nerve may be concerned.

Shoulder group

N. thoracodorsalis is derived from the extreme posterior part of the plexus in *Necturus*, the anterior part in *Iguana*, and the middle part in mammals. The reason for this phylogenetic shift is obscure. In mammals it arises chiefly from C. 7, and usually partly from one or the other of the neighboring nerves.

Nn. subscapularis, usually double or at times occurring as four or perhaps more branches, typically arise from the three anterior nerves of the plexus, with some rather slight variation. They innervate mm. subscapularis and teres major, and it is possible that they represent two nerve elements, as discussed under the musculature.

N. axillaris, innervating mm. deltoideus and teres minor, typically arises from the

first three nerves of the plexus, with a tendency to contract to two, at least in some carnivores (*Canis*) and *Galeopithecus*.

Brachio-antebrachial group

N. radialis. It is particularly difficult to determine the exact number of nerve roots involved in this and the median nerve. Perhaps it should be said that typically the former is derived from C. 6–T. 1, with slight extension, one way or the other in prefixed or postfixed cases. Its extent may be reduced, either mainly or completely, to two nerves (*Canis*). The radial nerve innervates the dorsal musculature of the arm, and it is never double, as in cold-blooded tetrapods.

Ventral (flexor) division

Infrazonal group

N. subclavius, to the muscle of the same name, is derived usually from the upper two, or at times probably only the uppermost, nerves of the plexus.

Shoulder group

Nn. thoracales anteriores form the anterior thoracic loop, and in mammals is composed of two elements—n. pectoralis proprius and a part of the n. coracoideus of lower vertebrates. The muscles supplied by the former element are mm. (1) pectoralis minor, (2) pectoralis abdominalis, and (3) panniculus carnosus; and by the latter, m. pectoralis major. In man we know that (1) is supplied by C. 7–8; but man lacks (2) and (3), and in other mammals it is impossible to analyze the components of the anterior thoracic nerve without degenerative experiments. It is known, however, that the typical condition in mammals is for mm. panniculus carnosus and pectoralis abdominalis to be innervated by the last two nerves of the plexus. Thus the fibers of the anterior thoracic nerves that represent n. pectoralis proprius have maintained their primitive position at the caudal border of the plexus. On the other hand, the part of the anterior thoracic nerve innervating m. pectoralis major, derived from n. coracoideus, arises in cold-blooded vertebrates chiefly from the most cranial root of the plexus, possibly with the next root participating. In consequence probably of the fact that this muscle in mammals is used in a variety of ways, its motor nucleus has increased in size to the point where the muscle is innervated by at least four and probably at times by five nerves of the plexus (except in particular cases, as Cetacea, in which but three nerves may be involved).

N. suprascapularis represents the remainder of n. coracoideus of lower tetrapods. The element, to mm. supra- and infraspinati, has maintained its original relationship, from the first, or first two, nerves of the plexus. It is definitely a ventral nerve element, but apparently because of the long affiliation of its musculature with the dorsal muscles of the shoulder, the peripheral part of the nerve at least is in process of alteration, so that grossly the nerve appears to be dorsal just as frequently as ventral in its relationship.

Brachio-antebrachial group

N. musculocutaneus, from the first two or three nerves of the plexus, is the first branch of the common flexor nerve of the arm. It is a consolidation of n. coracobrachiales of lower tetrapods (sometimes distinct in mammals also), and the common flexor fibers that innervate mm. biceps brachii and brachialis.

N. medianus and *n. ulnaris* should be considered as two branches of the common flexor nerve to forearm and hand. The former supplies most of the muscles of the forearm and several in the hand,

while the latter supplies two or more elements upon the ulnar side of the forearm and most of the hand muscles; but the innervation of several of the hand muscles may be by one or the other, for the terminal branches of a nerve are variable. Occasionally (*Hylobates*) all the three flexor nerves of the arm, except for their terminal branches, are confined in a single sheath.

MUSCLES

Because of limitation of space the musculature of the shoulder cannot be treated in great detail, with complete description of the conditions in all mammals, but only the variations occurring, with mention of particular mammals in illustration. Similarly, it will be impossible to give credit to each of the authors who have reported upon the muscles of the region. Probably the best course to follow is to rely upon my own dissections as much as possible, and to list in the bibliography only those papers on myology, annotated where necessary, that have been used in preparation of the text, but without reference to the authors. The details that appear to be most significant will be included, trivial items omitted. A serious difficulty is presented by the fact that muscles often are ambiguously described, have been overlooked, or misinterpreted. Also it must be realized that general statements can be made only tentatively. Thus it may be said that a condition is general in Carnivora, whereas the dissection of additional species may prove the statement false.

Branchiomeric musculature

The accessory field musculature should be considered as having developed from a single sheet, and occasionally (as in *Allactaga*) the sternocleidomastoid is separable from the trapezius only at origin.

M. trapezius is present in all mammals except the Odontoceti. Fundamentally it is a single muscle arising from the middorsal line and lambdoid crest caudally to the posterior thorax, and inserting upon the lateral clavicle, acromion, anterior spine and the dorsal part of the posterior border of the scapular spine. Because of the diversity of direction in which the fibers extend and the fact that it is the most frequently used muscle of the shoulder, it tends often to separate into three parts—clavotrapezius, acromiotrapezius, and spinotrapezius. The facility with which these parts are separable depends upon the ways in which the muscles are used and the feature, without phylogenetic significance, is variable within individual orders. Usually the distinct parts are but slightly separated, although the hiatus at times is considerable (Pinnipedia, *Canis*). In the Mysticeti only is it severely restricted to the cervical region and arises from the cervical fascia; or it may be so extensive as to overlap the cleidomastoid (*Gymnura*, *Centetes*, some Carnivora).

So far as I know, an occipital origin is lacking in no order except Dermoptera, and it appears to be uniformly present in Insectivora, Chiroptera, Primates, Tubulidentata, Proboscidea, Hyracoidea, and Sirenia, although in some descriptions it is difficult to determine whether an occipital trapezius or a cleidomastoid is meant. In the remaining orders it is variable.

Possible confusion exists in the literature between the clavicular insertion of the trapezius and cleidomastoid. An insertion upon the clavicle, or its raphe in aclaviculates, is present in Chiroptera, Primates excepting gibbons, and Rodentia possibly with a few exceptions; and it should be considered as present in those orders having a cephalohumeral. It is absent in Dermoptera, Cetacea, and

Tubulidentata, while the feature is variable in marsupials, insectivores, and edentates.

Occasionally (*Zalophus*) insertion includes the presternum, and in particular cases (Mysticeti, Phocidae) extends to the tuberosities of the humerus, although usually where a humeral insertion is mentioned (as in *Dasyurus*) one suspects the involvement of the deltoid. An interesting situation occurs in the Microchiroptera, in which a part of the trapezius—the occipito-pollicalis—extends to the neighborhood of the thumb. In the pocket gophers (probably in all Heteromyidae) (Hill), and moles (Campbell, MS) a curious cutaneous retractor muscle is formed by fusion of a thin sheet of the trapezius with a portion of the platysma. In *Tatusia* a separate slip of this field, from the cranium, fuses with the panniculus sheet, and a similar situation should be sought in all mammals with particularly complicated panniculus system.

Perhaps the most noteworthy condition of the trapezius is that in which the anterior part of the sheet combines with the cleidomastoid to a variable degree and these fuse with the clavodeltoid to form a cephalohumeral (or mastohumeral) muscle, continuous from the mastoid region to the humerus. The identity of the units involved is determined by the innervation, and it is a logical development of those mammals needing vigorous protraction of the forelimb. Hence it is usually present in cursorial forms and those using the pectoral appendage for aquatic propulsion, as well as in some fossorial sorts. A cephalohumeral is present in Edentata excepting *Cyclopes* and *Chlamyphorus*, in Carnivora, Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla, and in many of them it is extremely robust. In Hyracoidea the deltoid part of the complex extends its insertion to the ulna, in *Equus* to the fascia of the forearm, and in *Viverra* to the tendon of the brachialis.

M. sternocleidomastoideus. This should be considered as a single complex representing the cranioventral margin of the accessory field, which often splits into several parts. Fundamentally it seems that these should be considered as consisting of sternomastoid-sterno-occipital, and cleidomastoid-cleido-occipital portions, but the first of these pairs is uniformly termed sternomastoid, even if its fibers encroach on the occiput. As might be expected, the distinctiveness of the three recognized divisions is extremely variable, and one or the other may be partially or totally suppressed. At least among the larger orders, the insectivores are the only ones in which a cleido-occipital seems to be always present, and it is present in some members of all the others except the bats. It inserts for a variable distance along the lateral part of the lambdoid crest.

Perhaps the cleidomastoid should be considered as present in all mammals, although at times it is rudimentary (some bats), with origin scarcely encroaching upon the clavicle (or clavicular raphe). At times (*Bradypus*, *Bos*) it arises from the first rib as well as the clavicle, from the scapular spine (*Cyclopes*), or even from the humeral head (some Cetacea), although when origin appears to be from the humerus the deltoid usually is involved. It may be fused with the sternomastoid (as in some Marsupialia, some Chiroptera), more often, perhaps, is perfectly distinct, or may occur double (sciuromorphs).

In aclaviculate mammals the cleidomastoid, and cleido-occipital when present, customarily fuse with the clavodeltoid, usually as a single muscle from the head to the humerus, and the cranioventral part of the trapezius may be involved in this. Occasionally the condition is

too complex for precise analysis, as in *Zalophus*, which appears to have a distinct sternomastoid, two muscles from presternum and deep pectoral to the occiput and nuchum, both joining an additional cephalohumeral portion. Leche (1900) considered that the cephalohumeral is basically divisible into acephalohumeral proper, formed by the fusion of cleidooccipital with deltoid, and a mastoideohumeral, comprising cleidomastoid and deltoid. This scheme is useful to bear in mind but in many cases it is difficult to apply. In *Equus* both parts are fused, but in *Tapirus* only the mastoideohumeralis is present, and this inserts upon the deep pectoral (Campbell). Or, in Artiodactyla, the two are separate at origin but later fuse. *Manatus* and *Hyrax*, at least, are said to have only the cephalohumeral portion. The statement of Leche that in the horse there is fusion of the complex with splenius capitis and complexus minor, and of Sisson that attachment is also to the upper cervical transverse processes, are misinterpretations and refer to the levator scapulae ventralis.

Typically the sternomastoid extends between sternum and mastoid process, but it is subject to considerable variation. Usually it is quite well separated from its antimere, but there may be fusion in the midline (*Centetes*, some Carnivora), some of its fibers may contribute to the mastoideohumeralis, or it may be double (*Hyaena*, *Proteles*, some Chiroptera). It may have extensive origin from a midline raphe cranial to the sternum (*Potomogale*, *Phoca*), or arise partially (*Potomogale*) or wholly (Proboscidea) from the first rib.

The cranial attachment is usually the same for sterno- and cleidomastoid. Frequently it is upon the paroccipital process instead of the mastoid, or may be merely upon the undifferentiated surface of an enlarged audital bulla (*Dipodomys*). In mammals of ungulate affinity it frequently extends partially or wholly to the angle of the jaw, even piercing the parotid gland (Sirenia). In swine, hippos, sheep and goats, however, the paramastoid only is involved; it goes to both paroccipital and the angle of the jaw in Perissodactyla, Hyracoidea, and in Artiodactyla may even extend over the masseter muscle, or to the zygoma in Proboscidea.

Innervation.—The accessory field of musculature is supplied by the spinal accessory nerve, and in addition by one (usually 2) or more of the spinal nerves from C. 2 to C. 4, except in the long-necked ungulates (giraffe, camel, llama), in which the spinal accessory nerve at times is absent.

Myomeric musculature

Infrahyoid division

M. omohyoideus is the only muscle of this division of interest in the present connection. It is a unit of the ansa hypoglossi group, or ventral musculature of the neck, whose origin has migrated from the sternum along the clavicle to a final attachment, either to m. supraspinatus or bone, along the cranial border of the scapula, anywhere from the vicinity of the notch (most mammals) to the coracovertebral angle (*Didelphis*). Reflecting its derivation, it is frequently attached to the sternum by a fascial, tendinous, or even muscular, sling near its middle. It may be divided into two bellies by a stout, round tendon, which varies to the condition of complete absence. Parsons (1898) has suggested that the two-bellied condition has been brought about by a broadening of the shoulders (higher primates, bats), but this seems doubtful. Occasionally the insertion shifts from the hyoid to the root of the tongue (*Phascolarctos*) or the neighborhood of the symphysis menti (*Bathyergus*), and in such cases one suspects

that there has occurred a fusion with some suprahyoid slip.

The omohyoid is present in Marsupialia, Chiroptera, Primates (except some platyrrhines) and Perissodactyla; absent in Dermoptera, Proboscidea, Tubulidentata, and Hyracoidea. In Cetacea it appears to be present in Mysticeti and absent in Odontoceti. Among insectivora it is lacking in Talpidae, Chrysochloridae, *Solenodon* and *Myogale*. It is found in *Zalophus* but not in *Phoca*. In the Carnivora it is absent in Felidae and Canidae, present in Ursidae and Mustelidae, and variable in the other families (Windle and Parsons). It is variable, but mostly absent in Edentata. Among rodents it is usually present in the Sciuromorpha and Hystricomorpha, absent in Lagomorpha, and variable in Myomorpha. In Artiodactyla a curious situation obtains, for the more caudal portion is lacking in deer, giraffes, camels, and some sheep, and the cranial part fans out over the cervical fascia.

Innervation.—So far as exactly determined, by C. 2 and C. 3 via the ansa hypoglossi, possibly with some slight variation.

Dorsal (extensor) division of the appendage

Suprazonal matrix

The basic detail of this group is the serratus anterior. In the ancestral condition it expanded to include all the cervical vertebrae, and in this form it is encountered in many mammals. In many others, however, a part has dropped out—either the anterior cervical, posterior cervical, or slips from the middle ribs. The question of nomenclature therefore intrudes. By common usage the part from the anterior cervical region is termed levator scapulae (dorsalis). If origin be from the posterior cervicals only, what then? Or if the sheet from the ribs occurs in two portions, what should these be called? New names for muscles are not readily established, and any solution is arbitrary with the investigator. To my mind the proper course is to call the whole sheet serratus anterior, pars cervicis and pars thoracis. When an anterior cervical slip is separated this should be called levator scapulae dorsalis, and when the thoracic portion occurs in two divisions, these may be known as partes anterior and posterior. It is wiser to consider that the levator scapulae must always arise from cervical vertebrae, the neighboring fascia, or basi-occipital, and never from the lambdoid or mastoid. Because of the way in which the supraspinous fossa has developed it seems clear that the primitive point of insertion of the levator scapulae was in a position corresponding to the dorsal root of spina scapulae. At times it occupies this position (*Phascolomys*), but more often it has shifted to the coracovertebral angle of the bone. Basically, however, a part of the levator separated and its insertion migrated ventrally along the spine to the neighborhood of the acromion, and where it still occurs it is known by a great variety of names, but a desirable term is levator scapulae ventralis, as indicating its derivation.

The rhomboideus almost certainly arose by a spreading of the posterior serratus fibers to attach to the middorsal line, and thence by expansion cranialward, as is indicated by the situation in some of the cold-blooded tetrapods.

Mm. serratus anterior et levator scapulae, as indicated, should be discussed together. Typically they arise as a single sheet from the transverse processes of most, if not all, of the cervical vertebrae, and most of the ribs, the exact number being too variable, even individually, for consideration in this connection. Insertion is typically along

the medial aspect of the entire vertebral border of the scapulae, the parts to the two angles, chiefly the glenovertebral, being more robust than the rest.

This sheet is continuous or practically so in most Marsupialia (not phalangers, and beginning anywhere from C. 1 to C. 4), Insectivora (overlapping in some, as in *Chrysochloris*, and beginning C. 2 or 3), Dermoptera (C. 3), Primates (excepting higher forms, and in them including C. 1), many Rodentia (C. 1), most Carnivora (mostly C. 3, seldom from atlas (*Galera*)), Tubulidentata (C. 2), Hyracoidea (C. 3), Sirenia (C. 1), Perissodactyla and Artiodactyla (both C. 1, 2, or 3), with some variation from the figures as given.

Among orders having the sheet in two parts are the Edentata (fused in *Manis*), mostly with the levator beginning at C. 3 or 4, but at C. 1 in *Cyclopes* and C. 6 in *Bradypus*. In the latter type it seems that the levator should be considered as absent, and there is an anterior and posterior part of the serratus, with a hiatus between. This is the condition usual in Chiroptera also, but at times these may show origin as far forward as C. 3. In Pinnipedia the parts are separable but adjoin, or even overlap, from approximately C. 3 to T. 3 and T. 3 to 10 or 12. In Cetacea there is a restricted levator (C. 1) and a serratus from several (3 or 4) ribs; while in Proboscidea the levator is similar (C. 1), but the serratus is double (T. 1–5 and T. 7–10).

At least occasionally (*Gymnura*) a slip of the levator may occupy its primitive insertion upon the dorsal root of the spine and the rest shift to the coracovertebral angle.

M. levator scapulae ventralis (atlanto-scapularis inferior, omotrachelien, omo-atlantic, atlanto-acromialis, cervico-humeralis, trachelo-acromial, acromio-atlantal, levator claviculae, occipito-acromial) is usually present and normally extends from atlas to acromion; so it appears to have been derived from the more superficial and higher fibers of the levator scapulae dorsalis. In the literature it frequently has been misidentified, and statements that it is absent, or present, are not always trustworthy. It has nothing to do with the spinal accessory musculature, but the fact that its attachment at the shoulder frequently joins that of the trapezius, and that occasionally the two secondarily fuse (Chrysochloridae), coupled with faulty dissection, has been responsible for the erroneous reports that it is at times (pig, peccary, brocket, sheep, hyrax) innervated by n. XI. Occasionally it arises from more vertebrae than the atlas (*Equus*, C. 1–4), or shifts caudally (Tubulidentata, C. 2–3; Chiroptera, C. 2–3 or C. 4–5; *Giraffa*, C. 5 or 7). It may have origin from the base of the atlas (*Gymnura*, *Erinaceus*), or even have a basioccipital attachment (*Cephalophus*, many Rodentia). It may insert upon the spine just dorsal to the acromion, extend to the fascia over the teres minor (*Hyrax*) or supraspinatus (Tabulidentata), or migrate to the clavicle (many Chiroptera and Rodentia and some Primates). An interesting condition obtains in the Perissodactyla, in which the muscle fuses with the deltoid to form the transversohumeralis, possibly with a small part of the cleidomastoid occasionally included. The same situation may be found to obtain in *Phoca*, in which the muscle inserts upon the greater tuberosity and deltoid ridge.

The muscle is said to be absent, or usually so, in Talpinae at least among Insectivora, *Plecotus* at least among Chiroptera, the higher Primates and *Perodicticus*, Edentata, possibly a few Carnivora, Proboscidea, sometimes in Cervidae and Giraffidae, and Sirenia (the cleidomastoid was called levator claviculae by Murie). This list is subject to revision, because of

ambiguity in the literature. The muscle may be present in Cetacea, also, but this is difficult to decide.

M. rhomboideus is extremely variable in its extent. The absence of a pars capitis is a character primitive for tetrapods, but it is very doubtful if this statement applies to Mammalia, and there probably has been a secondary regression in those forms in which the sheet does not reach the head. The muscle acts largely in synergy with the trapezius and its details may depend much upon its needs in thus functioning.

Levator scapulae and rhomboideus capitis are both developments, but from different borders, of the serratus anterior; thus their relationship is close. It is well, however, to have a criterion for differentiation. Perhaps the rhomboideus *can* shift to the atlas, but there is no evidence that it does so, and it seems logical to consider that a slip from the upper cervicals or basioccipital to the scapula (or clavicle) is to be considered as levator scapulae, and one from the lambdoid area, rhomboideus capitis.

Few generalities can be offered. Origin is usually upon the midline, at times extending to the lateral lambdoid area, and insertion upon practically the entire vertebral border of the scapula, occasionally (as in Proboscidea, *Phoca*) being restricted to the glenovertebral angle. A single sheet from thorax to head occurs in Marsupialia (not reaching occiput in *Dasyurus*), Tubulidentata, many Rodentia (separable into two parts in others), and most Carnivora (Felidae, Procyonidae, Canidae, Mustelidae, usually Ursidae, seldom in Viverridae, Hyaenidae, with separate occipital slip in Mustelidae (Windle and Parsons)). In Pinnipedia there is a capitis (barely in *Phoca*) plus cervicis separable from a dorsi part. Among Primates, prosimians and monkeys have a pars capitis separable from pars cervicis and pars dorsi, but the first is usually absent in anthropoids and man, and the pars cervicis (minor) may retreat caudally. *Hyrax* has three divisions, dorsi major, dorsi minor, cervicis plus capitis. A single sheet restricted to the thorax occurs in Chiroptera, Dermoptera, blended cervicis and dorsi in Artiodactyla (but to head in *Hippopotamus* and Suidae), while in Perissodactyla pars cervicis may be blended or separable. In Proboscidea separable major and minor are confined to the thorax. The Edentata are extremely variable in all respects. The Cetacea are usually considered to have a small pars dorsi only, but the mastoscapularis of Odontoceti appears more likely to be a specialized rhomboideus capitis than a levator scapulae ventralis.

The slip called rhomboideus profundus by Clark, Dobson, Leche, Macalister, and Meckel (*Chironectes*, *Gymnura*, *Tupaia*, Mustelidae, *Lepus*) seems clearly to be a part of the levator scapulae.

Innervation. The long thoracic nerve supplies the serratus anterior, and short cervical nerves—usually all but the first, except in those with levator element arising well caudalward—the remainder of the complex. Almost always C. 5 supplies m. rhomboideus.

SHOULDER GROUP

Thoracodorsal matrix

M. latissimus dorsi is constantly present in all tetrapods. The extent of origin is variable and not of particular significance whether it arises from spines, ribs, fascia, or all three. There is a tendency in forms needing strong retraction of the forearm for origin to include the iliac crest (in swimming (*Desmana*), climbing (*Sciurus*) flying (*Vampyrus*), and brachiating (great apes)), but the character is not uniform. The different orders may be briefly sum-

marized as follows: Marsupialia, very variable in origin and insertion mostly to teres major, but at times separate, inserting below it (*Dasyurus*) or above (*Didelphis*); Insectivora, variable, to teres major in many, plus a slip to the bicipital groove in *Tupaia*, *Centetes*, with marginal fusion with pectoralis in *Chrysochloris*, and two layers in *Talpa*. When insertion is separate it is situated unusually far distally on humerus; Dermoptera, fusion with teres major; Chiroptera, from spines but not from ribs; Primates, costal origin usually lacking in prosimians and monkeys, and a partly scapular origin normal in man only; Edentata, insertion usually separate; Rodentia, variable, to teres in some but not in others; Carnivora, usually two layers near insertion, with anterior to dorsal panniculus and posterior to teres or humerus, the two parts straddling the biceps; Pinnipedia, in two parts, in *Zalophus* to teres border and to insertion of pectoralis profundus, but both to humerus in *Phoca*; in *Balaenoptera* to teres major, but in Odontoceti usually fused at origin with rhomboid, and partly divisible into two layers, neither to teres; Proboscidea, fibers twist and then split, to insert partly above and partly behind teres; Hyracoidea, splits, one part joining panniculus and the fascia over the biceps, and the other to the teres and a second part of the panniculus; Sirenia, normal to teres; Perissodactyla, poorly, and Artiodactyla, well developed. In the latter order insertion is usually to teres, but often with fibers continuing to pectoralis, with an extra slip of insertion in brocket, sheep, antelope (Windle and Parsons). At times (pig) there is a fibrous tract continues to forearm, or this may be fleshy (some Edentata) (see m. triceps), forming a dorso-epitrochlearis. The fact that the latissimus may straddle the biceps is of interest in connection with my previous discussion of the muscle in Caudata and Reptilia.

M. teres major. There is no distinct slip in lower vertebrates that can be homologous with this muscle in mammals. Hence one must conclude that it has either dropped out of living representatives of the lower classes, or that it was unrepresented in them and has differentiated *de novo* from the latissimus dorsi. The latter appears to be the proper explanation.

The teres major typically arises from the glenovertebral angle of the scapulae, regularly from the lateral aspect when phenomenally developed, when it may encroach on the medial surface and even fuse with the subscapularis, or fuse with the latissimus and triceps longus (Tubulidentata). Occasionally it covers a considerable area (*Talpa*, *Balaenoptera*), or may occupy only the axillary border of the glenovertebral angle (*Zalophus*). In many mammals its area of origin is indicated by the "teres major fossa," as a subdivision of the infraspinous fossa, but this is not a safe criterion, as occasionally (*Zalophus*) this fossa is occupied by the teres minor.

Most of the salient points regarding this muscle are mentioned under the latissimus, but in addition it may be said that the insertion is straddled by the latissimus in Proboscidea; in Cetacea it may be fused with the teres minor (*Globicephala*); and in Rodentia it is almost too variable for generalities, but the insertion is often in front of the latissimus in hystricomorphs, behind in sciuromorphs, and in myomorphs frequently wraps around the latissimus tendon and has a common insertion with it.

Innervation: The latissimus is supplied by the thoracodorsal nerve, and the teres major by one of the lower subscapular nerves. The topography of the nerves of these two muscles and of the subscapu-

laris in Mammalia is so similar that their details tell nothing of interest in regard to their phylogeny.

Axillary matrix

The relationship of the deltoid and teres minor is extremely close, as indicated by their innervation. Primitively they arose as a single sheet from the membranous girdle. When the cleithrum disappeared the part arising from that settled upon the scapula, and the subsequent development of the supra- and infraspinatus displaced the muscle to the axillary border, where it now occurs as the teres minor.

M. teres minor has been reported absent in *Gymnura*, *Centetes*, *Talpa*, *Potomogale*, *Phascogale*, some Chiroptera, *Manatus*, Cetacea, and some specimens of *Bradypus* and *Tapirus*. It is true that it may not occur in some mammals, but often it is fused with adjacent muscles, particularly the infraspinatus, and cannot be detected except by its nerve supply. It is certain that in many, if not all, cases its reported absence is erroneus and due to faulty dissection. It may be extremely small, from the neck of the scapula only (Proboscidea, Hyracoidea, *Equus*, *Balaenoptera* reported as subdeltoid), from most of the axillary border of the scapula as usual, spread to originate partly from the scapular spine, or metacromion (*Dasyurus*), occupy the "teres major fossa" (*Zalophus*), or occur as the most hypertrophied muscle of the shoulder and cover the entire infraspinatus (*Tupaia*). Insertion is usually upon the greater tuberosity just distal to that of the infraspinatus, but not infrequently it is partly upon the capsule of the shoulder joint, or it may join the deep tendinous belly of the deltoid (*Phoca*).

M. deltoideus, originally from the clavicle only, has spread to acromion and scapular spine in mammals and now occurs either as three separate divisions, cleidodeltoideus, acromiodeltoideus, and spinodeltoideus, or in various combinations of complete or partial fusion. It may be said that the former condition obtains in Chiroptera, Dermoptera, Perissodactyla, and the majority of Insectivora, Carnivora, and Rodentia. The fibers from the clavicle usually insert lowest and those from the spine the highest. Occasionally (as in *Hyrax*) origin may be from the infraspinous fascia, or even the axillary border of the scapula (*Zalophus*, *Tapirus*), rather than from the spine, the scapular part may fuse with the triceps longus (*Sus*), or the medial margin of the clavicular head fuse with the pectoralis major (Chiroptera, prosimians). The cleidodeltoid is said to be absent in *Gymnura*, *Solenodon*, and *Manis*, as it is in *Phoca*; the spinodeltoid absent in *Chrysochloris*, *Tupaia*, *Tarsius*; and acromiodeltoid absent in many Rodentia and most Artiodactyla. The cleidodeltoid occasionally is completely (*Citellus*) or partly double, with one part joining the brachialis tendon (*Viverra*, *Citellus*), or a split insertion may embrace the biceps. Insertion may reach the ulna (*Hyrax*), and fascia of the forearm (*Equus*), join the biceps and so to the radius (*Bradypus*, *Orycteropus*, and many Artiodactyla), join both biceps and brachialis (Felidae, usually Viverridae, sometimes Hyaenidae and Canidae (Windle and Parsons)), or fuse with the brachioradialis (*Manis*, *Zalophus*).

When a cephalohumeral is present it is formed by the fusion of the cleidomastoid and usually cleido-occipital, with the cleidodeltoid, with or without a raphe at the line of junction. When a vestigeal clavicle is present, it is located deep to the muscle complex. The cephalohumeral is said to be present in some Marsupialia (as *Phascolarctos* and *Phalanger*), Edentata (except *Cyclopes* and *Chlamy-*

phorus), Carnivora, Pinnipedia (possibly the deltoid element lacking in Phocidae), Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla; but undoubtedly there are more exceptions than those mentioned above, and the complex should be sought in all mammals lacking a clavicle. In Perissodactyla the acromiodeltoid also fuses with the levator scapulae ventralis to form the transversohumeralis.

Innervation: The teres minor and deltoid are supplied by n. axillaris.

M. subscapularis. At least the more posterior part of the mammalian subscapularis clearly appears to be homologous with the muscle of the same name in Reptilia, and with the short slip of the axillary group in Caudata that I have termed scapulohumeralis. I have been unable, however, to abandon the belief that the anterior part has been derived by an encroachment onto the subscapular surface of a part of the supraspinatus. The two are often fairly continuous, and the subscapular nerves appear always to be at least double. Neither of these points carry much weight by themselves; but according to Dejerine there are two separated subscapular motor nuclei in the spinal cord, which, so far as I know, can be claimed for no muscle without dual derivation. Conditons in *Tatusia* (and to a lesser extent in some other mammals dissected) are extremely suggestive of this conclusion, for in this there are two parts of the subscapularis, the anterior being innervated by a nerve close to n. suprascapularis. Hence I regard it as likely, but as yet by no means certain, that the subscapularis has been formed by the union of two elements, a theory to which I can see no valid obstacle.

The subscapularis usually occupies practically the whole subscapular surface, although its area may be less (Hyracoidea, some Artiodactyla), or the muscle may be greatly hypertrophied (Chiroptera, Dermoptera, moles). Perhaps usually it is definitely multipennated, and occasionally this is carried to an extreme where the muscle is separable into a number of divisions (many Carnivora, Artiodactyla, Odontoceti). The occurrence of a subscapularis accessorius or episubscapularis is not unusual (some monkeys, horse (Testut), pig, cat, *Zalophus*, suggested in *Bradypus* and *Myrmecophaga*), and this character may possibly reflect the line of junction of the two possible muscle units contributing. Insertion is always upon the lesser tuberosity, although it may spread to the capsule of the joint.

Innervation: By two or more nn. subscapulares. The close association of these with the branches to teres major and latissimus probably has no greater significance than that all are of axillary group derivation.

ELBOW GROUP

This consists of the triceps complex, including dorso-epitrochlearis and anconeus. The derivation of the dorso-epitrochlearis and triceps longus is not entirely clear. There is considerable evidence (Amphibia) to suggest that originally fibers of the latissimus migrated down the arm to form a slip with triceps action. If this slip survived as a contributor to the triceps mass, then its innervation shifted to allocation with n. radialis in response to the law of fasciculation. At a later date (Mammalia) this process may have been repeated, to form the dorso-epitrochlearis. If this assumption be false, then the latter muscle must be merely a displaced slip of the triceps proper, a premise which I am loath to accept. Less doubt is attached to the history of the caput longum tricipitis. Clearly there was urgent need for a long triceps, as there was for a biceps, and both have been formed,

mainly if not exclusively, by migration to the girdle along a plane of fascia by slips of the original elbow matrices.

M. dorso-epitrochlearis is absent as a distinct muscle in Chiroptera, Cetacea, Sirenia, antelopes, man, and individually in the anthropoids. Sometimes it is represented merely by tough fascia. Normally it arises from the tendon of the latissimus and disappears over the elbow. Not infrequently it occurs in two, or occasionally more, parts, and then there is a question whether a particular slip may not be a subdivision of the triceps. In rare instances it may arise only from the axillary border of the scapula, but in the great majority of cases this shift is not complete, and origin is both from the bone (or teres major, some Insectivora, Carnivora, and pig; or infraspinatus, some Artiodactyla) and the latissimus (Perissodactyla, Proboscidea, Tubulidentata). Occasionally origin is partly from the panniculus (Proboscidea, *Galera*).

In man, which normally lacks a dorsoepitrochlearis, panniculus carnosus, and pectoralis abdominalis, there is not infrequently encountered an axillary sling or achselbogen muscle, which should be considered as a remnant of one or more of the above muscles, in various combination.

M. triceps longus is invariably present in Mammalia, extending from the axillary border of the scapula to the olecranon. Occasionally it arises from the entire length of the border (some Marsupialia, Viverridae, some Artiodactyla), but usually it is more restricted. It may be two-headed (*Thylacinus*, some Edentata), or occur in even three or four parts (Tubulidentata, *Tapirus*). In *Tatusia* a part of the origin has left the girdle and now arises from the dorsum. A similar arrangement in some mammals seems to have been mistaken for a part of the latissimus dorsi. An interesting condition obtains in Pinnipedia; in *Zalophus* origin is from the ridge between the infraspinous and teres major fossae, and thence to the glenovertebral angle; while in *Phoca* the origin of this part is similar, but with extension to the dorsal base of spina scapulae. In this genus there is a second head from the scapular neck.

Mm. triceps lateralis et medialis are usually distinct, occasionally with a third division, but they need scant mention in the present connection.

Mm. anconei should be included for the sake of completeness. They constitute distal extensions respectively of the lateral and medial triceps. One or both may be absent.

Innervation: By n. radialis. The dorsoepitrochlearis has been reported as innervated by n. thoracodorsalis but I have never encountered this condition and am unconvinced that this occurs.

Ventral (flexor) division

Infrazonal matrix

This matrix seems to be entirely unrepresented in Amphibia, but appears as a variable number of slips in living (podial) Reptilia, passing from the sternum and ribs to the coracoids. A comparable situation obtains in Mammalia, but it is difficult to state the basic arrangement of the complex, for all sorts of poorly defined combinations occur. The conoid and trapezoid ligaments are generally believed to be remnants of muscle slips of this group (Huntington). In mammals it is perhaps correct to consider that there are two divisions present, one from the sternum or ribs to the clavicle (subclavius), and the other with a similar origin but inserting upon the girdle—coracoid or acromion usually, farther dorsally occasionally—(sternoscapularis). Both will be considered together, for in many cases they are hardly separable.

Mm. subclavius et costoscapularis. Both divisions appear to be unrepresented in Cetacea, Sirenia, Pinnipedia, some Carnivora, camels, giraffes, and some Tragulidae, Cervidae, and Bovidae. The subclavius has been reported as absent in Hyracoidea, Myrmecophagidae and Manidae, and the costoscapularis as lacking in Chiroptera and some Insectivora; but this list is unreliable. In Rodentia the subclavius is present and the costoscapularis variable, in form and definition. The feature is also variable in Marsupialia, Edentata, and the more specialized Insectivora. Perhaps the most interesting condition is the extension to the supraspinatus fascia in *Didelphis*, *Thylacinus*, *Chrysochloris*, *Orycteropus*, *Perissodactyla*, and undoubtedly others.

Innervation: N. subclavius.

SHOULDER GROUP

Pectoral matrix

This consists of the muscles derived from the primitive m. pectoralis, and comprise the following three divisions.

M. panniculus carnosus is far too intricately variable for concise description here. Almost always, notably with the exception of the higher Primates, and some platyrrhines, as *Ateles* and *Lagothrix*, some part is present, usually a dorsal sheet, and often a ventral one in addition. The typical insertion is upon the humerus distal to the greater tuberosity. The dorsal part is best developed in *Erinaceus* and armadillos (and Monotremata). In many animals it extends onto the base of the tail. Often it has marginal fusion with the platysma, a fact that led the older anatomists to the erroneous conclusion that the two fields were phylogenetically related. In addition there may be marginal fusion with the latissimus and pectoralis major, as in *Orycteropus*, or with the pectoralis minor (*Hyrax*). In bats, flying lemurs and flying squirrels the musculature of the membranes is derived from the panniculus, and this element furnishes slips to control the marsupium in Marsupialia, of the prepuce in some Artiodactyla, and to compress the mammae in Cetacea. Small areas may become separated from the remainder, as over the hips in *Pedetes*. Occasionally the muscle occurs in two sheets (more in monotremes), as in *Hyrax*, and then separate insertions may bridge the biceps. Insertion may be partially or wholly upon the latissimus tendon (*Phalangista*, Hyracoidea, Artiodactyla), or occasionally extend to the fascia of the forearm (Pinnipedia); or it may even extend over the shoulder to the malar region (*Manatus*).

Mm. pectorales minor et abdominalis may be considered a muscle sheet that often separates into a part from the sternum or sternal ribs, and a part from the abdomen. The latter typically arises from the region of the umbilicus and is opposed by m. pyramidalis (Marsupialia) for the support of the abdomen, with much subsequent variation. As in the case of the panniculus, the pectorals vary too much for detailed description in the present contribution. In addition, there is no ready criterion for distinguishing pectoralis minor from major elements, and most authors reporting their dissections have not realized the distinctiveness of the two groups. Consequently descriptions usually are difficult or impossible of interpretation in this regard and generalities are unsafe.

It is probable that the true pectoral, or pectoralis minor, layer is represented in all mammals, although the costosternal part may have retreated and only the abdominal part exists in a number of mammals (some Edentata and Artiodactyla). There may be marginal fusion with the obliquus externus (Manidae). It is certain that frequently there are two layers.

Insertion is primarily upon the proximal humerus, but in perhaps most mammals the attachment has retreated to the capsule of the joint, either or both tuberosities (as in *Equus*), often the coracoid, or even the supraspinatus fascia (*Equus*).

Innervation: By fibers of the anterior thoracic nerve that have fused with those to the pectoralis major.

Anterior coracoid matrix

This consists of the muscles that have been derived from the original coracohumeral group, arising from the procoracoid. There is little or no doubt, I think, that the change in the position of the humerus, incident to the alteration from the reptilian to the mammalian posture, was the stimulus that started a part of this matrix in its dorsal migration that resulted in its becoming the supra- and infraspinati. The pathway was clearly to the ventral part of the cranial border of the scapula, a part of the muscle passing upon either side of the clavicular articulation, anterior to which the supraspinous fossa developed. The extreme cranial border of the muscle could well have encroached slightly upon the subscapular fossa, and I cannot help but believe that it did so, eventually becoming the more cranial part of m. subscapularis. This is but a tentative hypothesis, however, for satisfactory proof is as yet lacking.

M. pectoralis major. The more ventral part of the procoracoid musculature shifted its origin to the sternum and became the pectoralis major, in mammals over-riding the true pectoral or pectoralis minor stratum. The difficulty of precisely allocating all pectoral slips to the proper one of the two components has been mentioned. Where one of the elements is lacking it is more likely to be the major. This appears to be the case in Mysticeti, and some Odontoceti but not in others. It is variable in extent and number of slips, but encroaches on the clavicle in relatively few mammals, as in some Marsupialia, some Edentata and Insectivora, *Pteropus*, and the higher Primates except the orang. In *Potomogale* this part is represented but arises from the cleidomastoid. Insertion typically is on the pectoral crest of the proximal humerus, but it may shift somewhat proximally, partly to the deltoid and biceps (Tubulidentata), or wander to the fascia of the forearm (Hyracoidea, Sirenia, Perissodactyla, Artiodactyla). Where the latter condition obtains it is probably, but not certainly, the major rather than the minor division that is involved.

Innervation: By a part of the fibers of the anterior thoracic nerve.

M. supraspinatus. Usually the size of this muscle may be inferred from the appearance of the supraspinous fossae, with a few exceptions, as the Cetacea, in which the muscle is larger than the fossa. It is present in all mammals. Not infrequently (*Civetta*, Pinnipedia, Hyracoidea, Perissodactyla, Artiodactyla) it is partially or completely double, in which case the smaller division usually arises from the anterior surface of the scapular spine. The insertion regularly is upon the greater tuberosity, but some of the fibers may wander to the pectoralis minor (*Choeropsis*, *Sus*), or to the lesser tuberosity. The latter condition is marked in forms in which lateral movement or rotation of the limb is restricted and the spinati muscles are employed almost exclusively for protraction (Hyracoidea and ungulates), although the condition is suggested in *Zalophus*.

M. infraspinatus. Present in all mammals so far as known except *Talpa* and *Mogera* (Campbell, MS), although reported in the former erroneously. The

variability, except in size, is usually negligible, and insertion is invariably upon the greater tuberosity.

Worth mentioning in this connection are rare slips, separable near the axillary border (*Capra*), and m. intraspinatus, between the base of the spine and the greater tuberosity (*Macaca*). These appear to be inconstant even specifically.

Innervation: Mm. supra- and infraspinati are supplied by n. suprascapularis.

The possibility of a part of m. subscapularis having been derived from this matrix has already been discussed.

Posterior coracoid matrix

This comprises the coracobrachial division, arising from the true coracoid. In mammals three parts may be recognized, superficialis, medius, and profundus. In some Reptilia (*Iguana*) there are two main divisions, separated by the flexor nerve, each of which is subdivisible into two parts. Conditions suggest that in mammals the superficialis and the part of the medius in man that is superficial to the musculocutaneous nerve, are homologous to the superficial slips in *Iguana*, while the part of the medius deep to the nerve and the profundus in mammals are matched by the deep slips in the reptile. This is a tempting explanation, but because the piercing of a muscle by a nerve is so easily accomplished by a gradual migration of fibers around the nerve trunk via a fascial plane, and the condition of piercing is encountered regularly only in man, it is wiser not to try to homologize the individual slips too closely. In the mammals so far dissected some part of the coracobrachialis is present in all forms except *Gymnura*, Talpidae, *Cyclopes*, Manidae, many Mustelidae, *Ictonyx*, Pinnipedia, and *Viscaccia*.

M. coracobrachialis superficialis. This muscle presents a difficulty. Very many times its presence is recorded in the literature under the name pars longa, but almost always it is an unusually long pars media that is meant. The criterion is that the pars superficialis, when present, always passes from coracoid to condyle superficial to all vessels and nerves (except the cutaneous ones) and all other muscles except the dorso-epitrochlearis. On these points the descriptions seldom are explicit. It appears to have been encountered in *Galera*, some rodents, some Artiodactyla, and occurs as an anomaly in *Pan*.

M. coracobrachialis medius is often reported as pars longa (superficialis), so is more frequently present than the literature indicates. It arises from the coracoid and inserts below the tendon of the latissimus for a variable distance, not infrequently practically to the condyle. Besides those mammals lacking all divisions, the medius appears to be absent in most Carnivora, some Edentata, and perhaps Cetacea, in which it is no more than vestigial at the most. It is variable in Marsupialia and Rodentia, being absent in some and present in others.

M. coracobrachialis profundus (brevis) is frequently overlooked by inexperienced dissectors. It arises from the coracoid and inserts always above the tendon of the latissimus, so is deep and very short. Besides those mammals lacking any division, it is supposed to be absent in a number of Marsupialia, most Insectivora, Chiroptera, anthropoid apes and man, many Edentata, various scattered Rodentia, perhaps a few Cetacea, Tubulidentata, Proboscidea, Hyracoidea, Sirenia, individual Perissodactyla, and some Artiodactyla. It never fuses with pars media, at least at insertion.

Innervation: By n. musculocutaneous, or at times by a separate coracobrachial branch of the common flexor nerve.

BRACHIO-ANTEBRACHIAL MATRIX

Brachial group

M. brachialis. In basic condition this muscle appears to have been at least partially divisible into an extensive lateral head, and a more restricted medial one, going respectively to radius and ulna. The former arose, and usually still does so, from the back of the surgical neck of the humerus and spirals around the bone. The medial head usually arises just distal to the deltoid insertion, or not infrequently its origin forms a V with the lateral head, the two partly embracing the deltoid.

Aside from its possible absence in some Cetacea, the brachialis appears to be uniformly present in all mammals. The medial head is often much reduced, but it is difficult to decide whether it is absent, as is supposed to be the case in ungulates. The lateral head usually retains its high origin. The typical insertion in mammals is upon the ulna, but at times it goes partly (some Marsupialia) or entirely (some Insectivora (as *Erinaceus*), Sirenia) to the radius, or in some orders (Edentata, Perissodactyla, Artiodactyla), insertion is upon the radius in some cases, the ulna in others, or even both (*Bradypus*, *Tapirus*, *Choeropsis*, *Sus*). The insertion as stated in the literature, however, is not always trustworthy, because of faulty dissection. Not infrequently in Primates there is partial fusion distally with the brachio-radialis, and the innervation of a small part of the brachialis by the radial nerve, to be encountered in this order, should be explained on the premise that some of the fibers of the brachioradialis have become incorporated with the muscle.

M. biceps brachii. This muscle is one of great importance in tetrapod economy, and different classes have had considerable difficulty in evolving it. In Mammalia, and lacertilians, it appears without question to have been derived by a division of the brachialis which reached the shoulder by migrating along a fascial plane. There it may have fused with a slip of the coracobrachialis, as it did in *Iguana*, so it is possible that the biceps, muscle and tendon, is a product of both divisions, although the muscular part of the mammalian biceps was derived from the brachialis only.

The condition of the biceps that was primitive for mammals appears to have comprised two separable muscles, a coracoradial and glenoulnar, as is commonly encountered in Marsupialia. These may fuse, and one head or one insertion be suppressed, thus accounting for the variation existing in mammals. It is generally considered that when but one head occurs it is always the longum, this argument being based on the fact that the tendon of the remaining head usually pierces the capsule of the joint; but it does not always do so, and the argument is weak. Many times two heads have a continuous origin, or a single head may arise from the base of the coracoid or the glenoid border. Where but one occurs it usually means that both divisions have fused, arising from one spot or another as best suits function. If this be from the glenoid border, the curve of the tendon over the joint usually results in its partial piercing of the capsule.

Similarly, one or the other of the insertions may disappear, or one (usually the ulnar) shift from the bone to the intermuscular septum, finally reaching the surface of the forearm to become a lacertus fibrosus (some primates, some ungulates) or a lacertus carnosus (gibbons).

A single biceps is encountered in a few Edentata, most Carnivora (except Ursi-

		NECTURUS	RANA	IGUANA	ORNITHORHYNCHUS	MAMMALIA
	Branchiomeric mm.	trapezius	trapezius	trapezius sternocleidomast.	trapezius (2) mesocleidomast. cleidomast.	trapezius cleido-occip. sternocleidomast.
DORSAL (EXTENSOR) DIVISION	Suprazonal group	serrati levator scap.	serrati rhomboid. levator scap.	serrati scap. levator scap.	serrati scap. rhomboideus levat. scap. dors. levat. scap. vent.	serrat. anter. rhomboidei levat. scap. dors. levat. scap. vent.
	Shoulder derivs. — thoracodors. matrix	latissimus	latissimus	latissimus	latissimus teres major	latissimus teres major
	Shoulder derivs. — axillary matrix	dorsalis scap. proscap. hum. long. proscap. hum. brev. scapulohum. brev.	dorsalis scap. delt. superf. delt. prof. scapulohum. brev.	dorsalis scapulae deltoideus proscap. hum. subscap. subcoracoid.	teres minor scap. deltoid proscap. hum. clav. deltoid subscap.	teres minor deltoideus absent absent post. subscap.
	Elbow derivs.	dorsitriceps coracotriceps	scap. triceps	scap. triceps corac. triceps	dorso-epitroch. scap. triceps	dorso-epitroch. triceps longus
		humerotriceps	humerotriceps	humerotriceps	humerotriceps	humerotriceps triceps anconeus
VENTRAL (FLEXOR) DIVISION	Infrazonal group	absent	absent	costoscap. sternocor. super. sternocor. infer.	costocoracoid. sternocoracoid.	subclavius costoscap.
	Shoulder derivs. — pectoral matrix	pectoralis	pectoralis	pectoralis	pannic. carnosus pectoralis	pannic. carnosus pect. abdom. pect. minor
	Shoulder derivs. — anter. cor. matrix	coracohum. major coracohum. minor	coracoradial cor. hum. epistern. cor. hum., cor. acrom.	coracohum.	coracohum. superf. coracohum. interm. coracohum. prof.	pect. major infraspinatus supraspinatus anter. subscap. (?)
	Shoulder derivs. — post. cor. matrix	coracobrach. glenoantebrach.	coracobrach. (3)	coracobrach. superf. coracobrach. prof.	coracobrach. superf. coracobrach. prof.	coracobrach. superf. coracobrach. medius coracobrach. prof.
	Elbow derivs.	brachialis	caput super. flex. carpi rad.	brachialis biceps	brachialis biceps (2)	brachialis biceps
	Hypobranchial mm.	hypobranch	hypobranch	hypobranch	omohyoideus	omohyoideus

dae and scattered genera), Pinnipedia, Tubulidentata, Proboscidea (individual exceptions), Hyracoidea, Sirenia, Perissodactyla, Artiodactyla, and some Lorisidae. Insertions in these are about equally divided between radius, ulna, and both. Orders that are particularly variable in all respects are Insectivora, and Rodentia (usually, however, a glenoulnar division is present). Not infrequently an additional humeral or capsular head occurs, as seems to be usual in *Bradypus* and as a variant in others, and a humeral head replaces caput breve in the gibbons. In the Talpidae the muscle is specialized in bizarre fashion, passing over trochleae so as to occur in three planes.

Innervation: The brachialis and biceps are supplied by n. musculocutaneous, and the former may receive fibers of n. radialis in addition, as already explained.

For the convenience of the reader a summary of the muscle homologies in the tetrapods discussed in the several parts of the present contribution is given in the table on page 461.

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ESSAYS ON EVOLUTION

I. ON THE EFFECTS OF SELECTION ON MUTATION RATE

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NEW recessive lethal mutations arise constantly without any artificial treatment. The results of Muller (1928) and others summarized by Schultz (1936) indicate, for *Drosophila melanogaster*, a frequency of about 0.18 per 100 gametes in the X chromosome, and of about 0.5 per 100 gametes in the second chromosome. It is probable that the third chromosome gives about the same frequency as the second; we may conclude that the normal frequency, for all chromosomes taken together, is about 1 per 100 gametes. Timoféeff-Ressovsky (1934) shows that there is also a high frequency of new mutant genes that cause an appreciable reduction in viability. In the X-rayed X chromosomes that he studied, such viability mutants were from 2 to 2½ times as frequent as were lethals. There is also a class of mutations that are lethal or reduce viability even when heterozygous. It is difficult to judge the frequency of the dominant lethals, but there is no doubt that they are present. Perhaps the largest class of dominant genes that lower viability is that of the minute-bristled types, known to be rather frequent. Finally, there is another type of mutation, as yet little studied, that brings about sterility (Berg, 1937).

In general, therefore, one may conclude that each normal, untreated individual of *D. melanogaster* is subject to a lowering of its potential reproductive value through the occurrence of new mutations in its germ tract. Quantitative estimates of this reduction must be approximate, since the recessive autosomal lethals and viability and sterility mutants will have an unpredictable effect. This arises from the fact that they operate only when homozygous, and the frequency with which they become homozygous will depend on the degree of inbreeding—a factor not yet evaluated for wild populations of *Drosophila* or any comparable organism. It is also to be observed that these recessives will operate only after recombination has reduced the correlation of their distribution in the population with that of the other genes present in the individual in which the original mutation occurred.

It seems probable, however, that the loss in potential reproductive value for the average individual is not far from one per cent, taking into account not only the immediate offspring but also later generations.

A body of data has been gradually accumulating that shows that different strains may deviate significantly from the usual values for mutation rate. This was indicated by Muller (1928), and has more recently been shown by Dubrolskij (1935) for material treated with X-rays and with heat, and by Demerec (1937) for untreated

material. Suggestions of similar differences may be found in still other papers (see Schultz, 1936). It appears that, for the X chromosome at least, some strains give lethal mutation rates as much as five or six times as high as those shown by other strains. Demerec reports that in the strains studied by him the chief difference was due to something carried by the second chromosome, though the new lethals studied were themselves in the X.

There are, then, genes that affect general mutation rate, and stocks differ in their constitution with respect to such genes. It follows that, in wild populations, such genes must be subject to selection. If a population has the mutation rate arrived at above, which leads to an average of 1 per cent decrease in the potential reproductive value of each individual, then a gene causing a 10 per cent decrease in the general mutation rate will have a selective advantage of 1/1000. The data suggest that decreases even greater than 10 per cent may be found; but a selective advantage of 1/1000 is sufficient to be effective (Haldane, 1924).

Any external agents that increase the general mutation rate will evidently increase the average loss in potential reproductive value, and will therefore increase the intensity of the selection for genes that lower the rate. Such selection will always be present, but it must be more intense in regions with high natural ionization, and also in regions with high temperatures. (For a critical review of the effects of temperature on mutation rate see Timoféeff-Ressovsky (1934a).) It may, therefore, be expected that, as a general rule, strains found in regions of high ionization and high temperature will have more uniformly low mutation rates (under standard conditions) than will strains found in regions of low ionization and low temperature. These relations will probably be difficult to verify experimentally; both because the sign of the selective process is the same in both situations, and because ionization intensity commonly shows such great local differences that it will be difficult to judge the intensity to which a population has been subjected.

It is probable that different strains will have accumulated different genes lowering the mutation rate, and that most strains (in cross-fertilizing organisms) will be somewhat heterogeneous for genes affecting that rate. If this be so, then any interference with the operation of the usual selection for low rate will have the effect of increasing the rate. The situation may be compared with that which obtains with respect to genes affecting the general viability of the organism. Under natural conditions selection keeps the members of a population up to par, but on relaxation of selection a slow deterioration sets in. Such deterioration is, as is well known, greatly speeded up by inbreeding. If the analogy here is valid, it may be concluded that any artificial conditions, maintained over a number of generations, are likely to raise the general mutation rate of a strain. Are all mutation rates so far determined experimentally too high to be applicable to natural populations?

The method of action of genes affecting the general mutation rate is still unknown. Probably they act in various ways; but it remains possible that they have something in common, such for example as an influence on the relative duration in time of specific stages of mitosis, or some other general property. In this case it might well be that such effects would have greater selective value through other results than through their effects on mutation rate. While this possibility must be kept open until more is known about the way in which the genes in question affect

mutation rate, it seems unlikely that it will invalidate the conclusions derived in this paper.

It seems at first glance that there should be a counter-selection, due to the occurrence of favorable mutations. It is true that favorable mutations furnish the only basis for improvement of the race, and must be credited with being the only raw material for evolution. It would evidently be fatal for a species, in the long run, if its mutation rate fell to zero, for adjustment to changing conditions would then not long remain possible. While this effect may occur, it is difficult to imagine its operation. It is clear that the vast majority of mutations are unfavorable, and Fisher (1930) has shown that the rare favorable ones must, in general, be genes with slight effects. In other words, for every favorable mutation, the preservation of which will tend to increase the number of genes in the population that raises the mutation rate, there are hundreds of unfavorable mutations that will tend to lower it. Further, the unfavorable mutations are mostly highly unfavorable, and will be more effective in influencing the rate than will the relatively slight improvements that can be attributed to the rare favorable mutations.

This raises the question—why does the mutation rate not become reduced to zero? No answer seems possible at present, other than the surmise that the nature of genes does not permit such a reduction. In short, mutations are accidents, and accidents will happen.

It may be noted that differences in general mutation rate may be characteristic of whole species. It has been the experience of all who have studied *Drosophila funebris* that it gives many fewer mutations than does *D. melanogaster* (see, for example, Timoféeff-Ressovsky, 1936). It has been suggested that this difference is due to failure of the same mutant gene to produce as distinct and easily seen a character in *funebris* as it does in *melanogaster*. My own experience leads me to disagree with this conclusion, since I found four types in *funebris* similar to known ones of *melanogaster*, and these four were at least as extreme and easily classifiable as the corresponding *melanogaster* types. How it has happened that *funebris* has come to have a lower mutation rate than *melanogaster* is not clear; but further analysis of this case may throw light on some of the questions raised in this paper. Possibly one might obtain more mutations by first crossing widely different races and then subjecting the hybrid lines to close inbreeding.

There are several characteristics of the organism that will influence the effectiveness of the selective process discussed in this paper. As in other types of selection, change will be more rapid in cases where selection can operate on haploid individuals, since here the large class of recessive mutations will be selected at once instead of only in the homozygous individuals. For this reason, the action of the suggested mechanism should be, in general, most marked in plants (where the gametophyte generation is haploid), and in animals, such as the Hymenoptera, with haploid males. In many animals and some plants there is one chromosome that is effectively (or actually) haploid in one sex. The number of genes that are carried by such a chromosome should show a positive correlation with the effectiveness of selection for low mutation rate.

Another general property that should influence this type of selection is the degree of inbreeding, since this will influence the rapidity with which unfavorable recessives are eliminated; and the sooner they are eliminated the greater will be the correlation between their elimination and

the presence of the genes that stimulated the original mutation.

In polyploid organisms the elimination of unfavorable recessives will be prevented or greatly hindered, and in polyploid plants the gametophytic elimination of unfavorable types will be greatly reduced; this condition should therefore raise the mutation rate. However, polyploidy also makes the detection of mutations more difficult, so experimental determination of this relation can hardly be expected.

One may surmise, then, that minimum mutation rates may be expected in diploid self-fertilizing plants (excluding balanced lethal types, such as *Oenothera*, which constitute a special case), and in lower plants, where the haploid generation is well developed. Maximum rates (per individual, not per unit of time) may be expected in cross-fertilizing animals that have no sex-chromosome mechanism or in which the sex chromosomes are relatively small. By way of examples, the rate should be low in Bryophytes and in legumes, high in Lepidoptera and in mammals—to cite groups that furnish familiar objects for genetic study.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

IRRTUM UND WAHRHEIT DER BIOLOGIE. *Kritik der Abstammungslehre.*

By Friedrich Andermann. Verlag für Medizin, Weidmann and Co., Vienna, Leipzig and Bern. Rm. 12. (paper); RM. 14. (cloth). 9¼ x 6¼; 282; 1937.

This is a work in the philosophy of biology, approaching the subject not with modern instruments of logical analysis and from the modern point of view of the Vienna Circle, so ably developed by Carnap, formerly of Vienna, and his associates, and by Ayer in England, nor from that of the English philosophers of science headed by Whitehead and Russell, but rather from the older idealistic standpoint, now happily falling into disrepute. Compared with the penetrating and fecund productions of Woodger, for instance, it stands in sorry contrast.

The particular biological topic it considers is the theory of evolution, which it attempts to refute as a whole. It is not concerned with one or another evolutionary theory, but claims to demolish the very notion of descent. Not only does it maintain that the concept of evolution is untenable in itself, but also that the theory of descent is incompatible with the erection of a systematic science of zoology and botany. Nevertheless one must not infer from this that Andermann favors a return to a religious explanation of the origin of species. He believes in the fixity of species, but not in religious fundamentalism.

Andermann modestly credits Constantin Brunner for the ideas herein expressed, and merely wishes to apply these ideas to the domain of biology. But to do so, the author finds it necessary to step beyond the boundaries of this domain and to soar in the dizzy atmosphere of metaphysical speculation.

The origin of species is especially subject to attack, and in particular the author rejects the historico-chronological approach of the biological scientist. The theory of evolution, he believes, contradicts the facts of experience and outrages reason. Only by misinterpreting the facts is the theory apparently based on comparative anatomy, embryology, and paleontology. Indeed, as far as experience and observation can tell, species are essentially constant. Species are fixed, with definite lines of demarcation between them, and every variation occurs within the species themselves. One does not merge or develop into another. Their existence rests upon a "super-individual cosmic reality" in which they are recognized as being manifestations of One unchangeable Being in which all appearances are rooted. That there exists such a great similarity of structure and function among species of animals (and plants) is because together they "constitute a basic element of Nature, the primeval phenomenon of Life."

This phenomenon expresses itself in countless variations of species all of which, however, are on an equal plane and are of equal value. Species can be arranged in order of complexity, but not in order of precedence, importance, or value. Nature, in short, has no favorites.

It is, of course, not entirely fair to treat an argumentative work such as this one in a few words, and to tear the more purple passages of philosophical fantasy from their context for the edification of readers of this review. Nevertheless, these passages are enlightening in that they clearly display the point of view of the author and the futility of his endeavor.

PALAEONTOLOGY. *Invertebrate. Seventh Edition.*

By Henry Woods. The University Press, Cambridge; The Macmillan Co., New York. $3.25. 7½ x 4¾; 475; 1937.

The classical work on paleontology is of course Zittel's textbook from which the present work differs in being smaller, and therefore cheaper and more portable. Also, it has been ten years since the last edition of Zittel appeared, so that Woods' work is more up to date, yet the taxonomy of the mollusca given by Woods is not so modern as that of Zittel.

On the other hand the introductory material in Woods is better expressed than that in Zittel, the discussion of convergence being particularly good. The genus *Gryphaea*, for instance, has been evolved separately three times, once in the Lias, once in the Oölite, and once in the Cretaceous, but there is no genetic connection among these stocks, as the strata in which they occur are separated by other strata in which they do not. It is a question, however, whether we should consider this as an example of real convergence, since each stock has been derived from the same origin, the genus *Ostrea*, which has existed from the lower Mesozoic to the present substantially unchanged. If *Gryphaea* had been derived from different origins, such for instance as *Aetheria*, *Chama*, or *Rudistes*, all of which bear a superficial resemblance to *Ostrea*, such a type of convergence would constitute a true involution, but there is no evidence that any natural group of species has ever had a polyphyletic origin. There is no apparent reason why the property possessed by one genus of evolving another should not be perpetuated through paleozoic periods like other generic characteristics, and forms so derived may appropriately be considered congeneric.

The index covers twenty-two and a quarter pages.

PALEOCENE FAUNAS OF THE SAN JUAN BASIN, NEW MEXICO. *Transactions of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. New Series, Volume XXX.*

By William D. Matthew. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia. $5.00. 12 x 9½; viii + 510 + 65 plates; 1937 (paper).

The San Juan Basin of New Mexico lies largely in the northwestern corner of that state, a small section extending over into Southern Colorado. The faunas, first reported by Cope in 1880, belong to the two earliest phases of the Paleocene epoch, a transitional period following that of the great reptiles of the Mesozoic era, and preceding the Lower Eocene phase when the direct ancestors of the horses, tapirs, ruminants, and other progressive placental families first became established in western North America. The present report, an important contribution to palaeontological literature, has been many years in the making (it was commenced in 1916) but one can scarcely find fault on this score in view of the high quality of the work and its completeness in every detail.

Two very distinct faunas are recognized: the Puerco (17 genera representing 7 families and 6 orders), and the Torrejon (26 genera representing 14 families and 7 orders). No species (of the two families)

passes through from one to the other unchanged; most of the genera are different, and in those genera that pass through (*Periptychus*, *Anisonchus*, *Ellipsodon*), the representative species in each horizon are very distinct, those of the Torrejon in all cases much more specialized. The genera of certain families, closely allied and primitive in the Puerco, are decidedly more specialized and divergent in the Torrejon.

The general character of the vertebrae, limbs, and feet are suggestive throughout of animals either of arboreal habits or derived from arboreal ancestry.

The volume includes a chronological list of publications dealing with Paleocene faunas of the San Juan Basin; a group of 65 plates giving many very fine illustrations of the material studied and a map of the region; and an index. It is unfortunate that the death of the author in 1930 prevented his seeing the completed *opus*, the publication of which the American Philosophical Society generously supported.

EVOLUTION OUT OF DOORS. *A Study of Sex Differences and Animal Coloration.* *By Henry J. C. Molony. Edited and with an appendix by J. R. de la H. Marett. Photographs by Cherry and John Kearton. Hutchinson's Scientific and Technical Publication, London.* 15s. net. 8½ x 5½; xx + 248 + 30 photographs; 1937.

The author considers evolution from four aspects, as follows:

Process A is the failure *as individuals* that are born with disadvantageous variations, or the elimination of the unfit. It is responsible for both concealing and bold colour patterns, and for an inherited immunity from many diseases. Process B is the failure *as parents* of individuals that are born with disadvantageous variations. Parents of certain types fail to leave the normal number of representatives in later generations and these types gradually die out. Process C is the failure *as mates* of individuals that are born with disadvantageous variations. It has produced the bright colours in flowers that are fertilised by pollen-bearing insects. Of course, the success of individuals born with advantageous variations could rightly be described as a case of natural selection; though how far this contributes towards evolution is doubtful. Albino and melanistic forms occur from time to time in most, if not all, species, both in the wild state and in domestication. In nature it usually happens that they disappear without influencing the race, but occasionally the change of colour seems to supply a long-felt want, and the new form flourishes and increases.

A very large number of facts, many of them gathered from the author's own experience, are drawn upon to support the theories that are advanced. The full page photographs are exceptionally good ones. There is a short bibliography, but no index.

GENETICS

THE EVOLUTION OF THE AUSTRALIAN MERINO. *By E. W. Cox. Angus and Robertson, Sydney, New South Wales.* 21s. 10 x 7¼; xxii + 160 + 31 plates; 1936.

An interesting book. In 1765 Spain first permitted exportation of the Merino. About thirty years later, a few of these remarkable sheep found their way to New South Wales and were bred with existing flocks. Early in the nineteenth century when it became apparent that the climate and grazing conditions of this continent were not only suitable to the well-being of the Merino but that the high quality and quantity of its wool did not deteriorate, a systematic effort was begun to bring to Australia the best of the various Merino strains for breeding purposes. This is the story of the successes and failures which have produced the Australian Merino of today—a type entirely different from the Merino in any other land and quite unsurpassed both in the quantity and quality of its wool. The numerous illustrations (entirely of sheep) show all the great Australian rams and the different evolutionary stages of the present day animal. The volume has an index.

ERBPATHOLOGIE. *Ein Lehrbuch für Ärzte.* *By O. F. von Verschuer. Verlag von Theodor Steinkopff, Dresden and Leipzig.* RM. 8. (paper); RM. 9.20 (cloth). 9¼ x 6½; vi + 213; 1934.

This book by an eminent geneticist should find a niche in the medical practitioner's library. About half the book is devoted to a concise discussion of the essential principles of human genetics. The second half presents short notations on the frequency of occurrence, mode of inheritance, records from the literature, and other pertinent matter of a long list of diseases and anomalies. These are arranged in a handy manner, alphabetically under headings such as nervous diseases, diseases of the inner organs, deformities. Included also is a brief treatment of blood groups, finger prints and other tests utilized in attempts to establish paternity. A bibliography and an index have been provided.

EXPERIMENTELLE MUTATIONSFORSCHUNG IN DER VERERBUNGSLEHRE. *Beeinflussung der Erbanlagen durch Strahlung und andere Faktoren.*

By N. W. Timoféeff-Ressovsky. Verlag von Theodor Steinkopff, Dresden and Leipzig. RM. 15 (paper); RM. 16.50 (cloth). 9 x 6; x + 181; 1937.

This monograph is intended as a brief and critical survey of the mutation work done since the first attempts of H. J. Muller to produce mutations in *Drosophila* by X-rays. By far the greater part of the book is devoted to the effect of X-rays on the chromosomes and the various mutations produced. Chapters 8, 9, and 10 deal respectively with temperature and chemical influences on mutations, the nature of mutations, and the usefulness of experimental mutations. There is an extensive bibliography and an index.

GENERAL BIOLOGY

ECOLOGICAL ANIMAL GEOGRAPHY. *An Authorized, Rewritten Edition Based on Tiergeographie auf oekologischer Grundlage.*

By Richard Hesse, W. C. Allee and Karl P. Schmidt. John Wiley and Sons, New York; Chapman and Hall, London. $6.00. 9 x 5⅞; xiv + 597; 1937.

Here is an example of coöperative book production. Dr. Hesse wrote the original work, Dr. Schmidt translated it, and Dr. Allee completely rewrote it, changing the order in which the material originally was presented, and altering it when in his judgment the work might be improved thereby. Thus the present work may aptly be compared to a revised edition.

The book is divided into four parts. The first deals with general ecology, the next three with the special ecologies of the ocean, the inland waters, and the land.

The oceans of the world are continuous, with little to obstruct the spread of species except local changes in temperature, while by way of contrast the various freshwater systems are so isolated from each other that one would suspect, if segregation be an important factor in evolution, that every river system would have its own endemic fauna. As is well known exactly the opposite occurs, and perhaps the most interesting part of the book is its discussion of fresh water faunas. The explanation is of course that while fresh water systems are isolated in space they are not so in time. Geological agencies are consequently making and unmaking river systems. The Great Lakes have had eight different outlets to the sea since their formation in the pleistocene. The Hwang Ho has had even more in historic times. Furthermore, lakes are only a temporary feature of physiography, and they fill up too rapidly for any considerable amount of evolution to take place in them. To develop an endemic fauna, a lake must have long duration in geologic time, and be isolated from other bodies of water. Such lakes are extremely few, and consequently endemic faunae are rare. The only lakes with endemic faunae are Lakes Tanganyika, Baikal, Ochrida, and three small lakes in Celebes, all of which are deep enough to offer real abyssal conditions. It is strange that no mention is made of Lake Lanao in the Philippines with its well known endemic molluscan fauna.

There is a chapter dealing with ecological changes brought about through human agency, which today outnumber all other ecological changes, and a comprehensive index of 41 pages.

Perhaps the most helpful feature is the style. It is necessary in discussing ecology to make use of a great many terms unfamiliar to any reader except the specialist in ecology, but these are all carefully explained in the text. The documentations are voluminous, sometimes running up to over 150 footnotes to the chapter. Everything considered, this is a work of great significance, and one that should be widely read.

GENERAL BIOLOGY.

By Leslie A. Kenoyer and Henry N. Goddard. Harper and Bros., New York. $3.50. 8½ x 5½; xxiii + 630; 1937.

A MODERN BIOLOGY.

By Ernest J. Holmes and R. Darnley Gibbs. Foreword by Ralph H. Crowley. The University Press, Cambridge; The Macmillan Co., New York. $1.32. 7¼ x 4⅞; xv + 272; 1937.

METHODS IN BIOLOGY.
By Alfred C. Kinsey. J. B. Lippincott Co., Chicago. $2.50. 8 x 5¼; x + 279; 1937.

ANIMAL BIOLOGY. *Revised Edition.*
By Michael F. Guyer. Harper and Bros., New York. $3.75. 9¼ x 6⅛; xx + 735; 1937.

The authors of *General Biology* have produced an excellent elementary textbook for the college student. The material is not original but has been carefully selected from a wide range on the basis of years of experience in organizing and teaching the subject. Outstanding features of the book are the balance maintained between the plant and animal types described, the large number of illustrations with their detailed captions, and the comprehensible presentation of an adequate physical and chemical background for the basic physiological processes. The appendices include an outline of classification of plants and animals, a glossary, and a list of references. There is also an index.

A Modern Biology was designed for high school seniors and intended to serve as a sound foundation for education "in the art of living the healthful life." This results in a somewhat unusual organization and emphasis of the material, although the material itself does not differ appreciably from that presented in other modern biology texts. The book is illustrated with drawings and photographs and contains an index.

Methods in Biology is a book for the teacher. It contains much good advice and many helpful suggestions and should prove useful to the veteran in a rut as well as to the novice.

The first edition of *Animal Biology*, a complete elementary text, was noticed in these columns in Vol. VII, No. 3. In the revised edition the synoptic survey of the animal kingdom has been expanded into an entire section, and there are other minor changes and a few additional facts.

LE VOYAGE DE LA PÉROUSE SUR LES CÔTES DE L'ALASKA ET DE LA CALIFORNIE (1786). *Avec une Introduction et des Notes. Historical Documents Institut Français de Washington Cahier X.*
By Gilbert Chinard. The Johns Hopkins Press, Baltimore. $3.00. 10¼ x 7½; xlix + 144 + 22 plates; 1937.

The study of geographic exploration is always fascinating, and such names as those of Marco Polo, Paul Knutsen, Fernando de Magellan, and Cabeya de Vaca never fail to stir the imagination. La Perouse does not exactly belong to this group, for he was not so much of a pathfinder as a pioneer; he had been preceded in Alaska by Bering and in California by Cook.

Much has been written of the exploration of the West Coast by the Spanish, the Portuguese, the English, the Russians, and even the Japanese, but it is not so generally recognized that the French, through the expedition of La Perouse, also had a large hand in this work. This is principally because the publication of La Perouse's narrative did not take place until the decade following that of George Dixon, although the latter did not arrive on the ground until a year later than his French contemporary. As a result, the earlier place names given by La Perouse have had to fall within the synonymy of those of Dixon by the law of priority.

To remedy in some degree this lack of knowledge of the French pioneer the French Institute of Washington has issued this beautifully prepared volume, containing the text of La Perouse's own narrative of his voyage from Hawaii to Mount Saint Elias, thence to Monterey and return, together with an extensive introduction. Here one can read the account of the storm which destroyed two of his ships and took the lives of twenty-one of his men, and the reader naturally wonders if during this experience he had any premonition of the fate that was to be his two years later, when he and all his band disappeared between Botany Bay and Vanikoro, never to be heard of alive again.

La Perouse's scientific interests covered the entire field of natural history. All three kingdoms of Linnaeus were represented in the collections made by his party. In addition he made an investigation of the general health of the Indians with whom he came into contact, made anthropometric measurements on them, prepared vocabularies of their languages,

discussed the history and economics of the Spanish missions in both Californias (although he himself never got south of Monterey) and took down some of their vocal music. Incidentally it may be noted that he reports part singing by the natives of Alaska. Similar observations have since been made by Chavez and Marcelli on the Indians of Chile and Peru, yet most historians of music state that part singing is indigenous only to the Keltic peoples of western Europe, and that wherever else it occurs it has been derived ultimately from this source. The matter is in need of further investigation.

The illustrations of scenery and of natural history objects are on the whole superior to those of people. Those of the Alaska Indians, for instance, indicate Nordic features with expanded lower lips, such as are commonly associated with certain aboriginal Africans. It is conceivable that the Alaskan Indians may have looked like that, but it seems most unlikely. This is no reflection on the present work which merely reproduces the illustrations from the original text.

The index covers four pages.

FAISCEAU ÉNERGÉTIQUE ET BIOLOGIE *Biogenèse et Pathogenèse. Pression Solaire.* *By G. Froin. Girardot et Cie, Paris.* 30 francs. 9½ x 6½; 327; 1937 (paper).

Using Hegel's phrase *Das Wahre ist das Ganze* as a title-page quotation, the author proceeds to explain the origin of life and disease by drawing upon chemistry, physics, neurology, physiology *and* astrology, etc. The author is difficult to follow in some of his arguments. Perhaps the best idea of his train of thought may be gained from the translation of a few excerpts from the last chapter entitled *Influence of the stars on life and terrestrial maladies.*

The red corpuscles live 42 days, that is 6 weeks. Their duration of life is governed for 4 weeks by the Earth and the Moon and by Mercury and Venus for the remaining 2 weeks.

Hematolysis is released by terrestrial magnetic pressure during the new moon and the first quarter. In 14 days the 6 magnetons that cover each surface of the red corpuscles are liberated into the blood. During the 14 days of the full moon and the last quarter the 6 photons that cover each surface of the red corpuscles are eliminated by the blood. There remain 24 noctons; 12 are eliminated by Mercury and 12 by Venus during the last 14 days. Such is the process of total hematolysis in the male, utilizing noctons and photons, liberated and very active, for the building up of spermatozoids.

For the growth of the foetus, the normal astral pressures are not sufficient. The nourishment of the foetal tissues by the generative nocto-magnetons is produced through the medium of the hypophysis, which fixes a large mass of these elements, absorbed by the eyes and the optic nerves. If they are predominant in the blood of the woman, and especially if they penetrate the placenta in great numbers, a male child develops. For that, it is necessary that together with a strong solar pressure an equally strong counter-pressure is produced on all the axes of the dihydrol through the medium of Jupiter. . . . If the pressure of Jupiter is less strong and if the photonic lines are activated by the Asteroids and the magnetic line by Mars, a less abundant migration of prolan into the placenta is produced and a female is created.

The author claims this to be the first book published to date on biological physics.

IN QUEST OF GORILLAS. *By William K. Gregory and Henry C. Raven. The Darwin Press, New Bedford, Mass.* $3.50. 9¼ x 6⅛; xvi + 241 + 111 plates; 1937.

Dudley J. Morton, at the College of Physicians and Surgeons of Columbia University, found from his studies on orthopedic problems that a great deal of both theoretical and practical knowledge could be acquired about the mechanism of the human foot by viewing it in the light of its long evolutionary history. The American Museum of Natural History could supply him with all the specimens he needed with the exception of adult gorillas and primitive unshod humans. In pursuit of the material lacking, the museum and Columbia University sent out an expedition under the leadership of Mr. Raven.

This is the story of their journeys and adventures in the mountains and jungles of Equatorial Africa. In a pleasant style the authors tell of the African towns, and of the lives of the natives with whom they came in repeated contact in their efforts to secure and photograph foot prints.

The tale of getting the gorillas, although at times daring and exciting, is by

no means entirely one of active adventure. The creatures, which were to be embalmed and sent home, had to be shot in the head only, as a body wound might cut many blood vessels and prevent the preserving fluid from reaching all parts of the body. As they had permission to shoot five specimens only, two in the Belgian Congo and three in West Africa, the hunters had to miss many chances of merely slaughtering their prey, and wait tedious weeks for precisely the right moment to kill.

Abundantly illustrated with many interesting photographs and an occasional sketch from Gregory's note-book, this volume is a charming account of a scientific expedition.

HANDBOOK OF MICROSCOPICAL TECHNIQUE. *For Workers in Animal and Plant Tissues. Second Edition, Revised and Enlarged.*

Edited by C. E. McClung. Paul B. Hoeber, Inc., New York. $8.00. 9¼ x 6; xvii + 698; 1937.

It is obvious that no individual person could gain, in the course of a normal life time, the experience required to work out with such precision and detail, the numerous microscopic techniques described in this volume. The editor gives us the very broadest general methods of preparing materials for microscopic study. He then turns the enterprise over to numerous specialists in the different fields of microscopic investigation, and each of them describes the methods and techniques that have evolved as a result of many years experience in a special field.

The book contains many illustrations provided to present the structure of pieces of apparatus and manipulative procedures. Because the volume is a hand book for practical use, there is no historical treatment of the subject, and generally only such bibliography as is necessary to complete an understanding of the indicated methods. It is assumed that the book will be of interest particularly to workers in bacteriology, botany, cytology, embryology, histology and pathology, but the presentation of general methods in Part I makes it useful to any student using the microscope.

THE LIVING WORLD.

By Samuel H. Williams. The Macmillan Co., New York. $3.60. 8½ x 5½; xxii + 704; 1937.

The teacher who has tried to organize a course in nature study knows the scarcity and consequent need of text books in this field. It has been the desire of the author to fill this need by giving us a text that he has prepared primarily for use in courses of nature study, field biology, and the elements of ecology. The book is organized around four large topics, namely; (1) The Biological Aspects of Living Things, (2) Animal Study, (3) Plant Study, and (4) Methods of Study. The author has approached the subject with emphasis on the method of study, rather than on types or forms of organisms as is generally the case. The method suggested and emphasized is that of the study of living things in their natural surroundings. Emphasis is also placed on the study of the relations and reactions of living things to each other, and to their surroundings, as observed in nature.

The book is well planned, well written, and well illustrated. Many chapters contain extensive bibliographies on the topic under consideration. There is an appendix of some two hundred questions and answers on general knowledge of the living world. The book will be of great value to every student and teacher of nature study, as well as to those few laymen who love and appreciate life in all of its various forms.

CODE UNIVERSEL DES COULEURS. 720 *Couleurs.*

By E. Séguy. Paul Lechevalier, Paris. 60 francs. 6¼ x 4⅜; lxviii + 55 plates; 1936 (paper).

Strictly speaking, this is not a book at all. It consists of forty-eight cards, each with fifteen colors arranged like the samples of a paint salesman. All the colors are blends of red, blue, and yellow, the proportions of which are constant on each card, but vary from card to card. Also, the colors are diluted with varying amounts of white and black, the proportions of white being constant in the hori-

zontal rows and those of black constant in the columns, except that in some instances other colors than black are used, the proportions remaining the same. There are also seven cards of solid color, each with a rectangular hole in the center, for the purpose of covering the other cards except for the one color that is being compared. There is also an explanatory booklet printed in French, with condensed summaries in six other European languages. Whether or not it is superior to Ridgway's Chart will be a matter of personal taste.

DAS GEFÜGE DES LEBENS.

By Ludwig von Bertalanffy. B. G. Teubner, Leipzig and Berlin. RM. 6.80; outside of Germany RM. 5.10. 9 x 6; iv + 197; 1937.

This book is a well written and stimulating discussion of the phenomenon of life, based on the principles of dynamic biology. A thorough study of the structure and functions of the cell, the physiology of metabolism, the manifestations of irritability, and the powers of growth and development, has shown the author that any living being is a system of fundamental activities, all working together for the existence of the ultimate well-regulated organism. The mathematical interpretations of certain aspects of life are in keeping with the author's well known organismal theory, and his devotion to exact biology.

The volume is well illustrated. The bibliography of 114 titles and the complete index are an adequate conclusion to the work.

UNIVERSITY OF COLORADO STUDIES, *Volume 24, Number 2.* Containing following articles: *The Birds of Boulder County, Colorado,* by Gordon Alexander; *Population Studies of the Trout of the Gunnison River,* by Harold Pratt; *Algae of some Thermal and Mineral Waters of Colorado,* by Mildred Hallberg Jones; *A Working Bibliography of Daylength and Artificial Illumination as Affecting Growth of Seed Plants,* by Francis Ramaley. *University of Colorado Press, Boulder.* $1.00. 10 x 6⅞; 48; 1937 (paper).

HUMAN BIOLOGY

ANTHROPOLOGY. *An Introduction to Primitive Culture.*

By Alexander Goldenweiser. F. S. Crofts and Co., New York. $3.75. 9 x 6; xxi + 551 + 30 plates; 1937.

Of all the various sciences that go to make up biology that most in need of systematization is anthropology, the science in which *Homo sapiens* becomes the studied object as well as the studying subject. The present work does not achieve the synthesis of ethnology and archaeology but it goes a long way in that direction. It is a work which may be profitably read several times, each time disclosing some new item of interest.

It is characterized by the inclusion of two chapters, which the author says are unusual for this type of book. The first is on how anthropologists work—a chapter that might well have been expanded into several. Many statements made by anthropologists do not carry conviction because the average man knows so little of the technique of the science. The anthropologist who discovers two cultures in widely separated localities and who asserts confidently that the one is older than the other may be absolutely correct in what he says, but except to those who know something of the underlying factors that led to the decision the simple statement is not likely to seem very convincing.

The need for the other chapter is not so apparent. It is an account of the contacts between the white man and the Indian and appears to have been written hurriedly with no attempt to verify statements. Thus in speaking of the activities of the present administration he writes "The response of the Indians to this opportunity for home rule has been enthusiastic, even overwhelming." As a matter of fact, the most uncompromising opposition to the innovations of the present administration come from the Indians themselves. It is true that the American bison has been saved from extinction by the

creation of bison ranges, but after all the Indian is a human being, and who can blame him for not wanting to be made into a living museum specimen on a reservation like the bison? While there is much in Indian culture that is superior to that of the white man, the fact remains that out of a population of about one hundred and thirty millions in the United States, the Indian (together with the mestizo) constitutes less than half a million, and if the Indian is to survive at all he must do so by becoming adjusted to the environment in which he must live; therefore the policy of Ex-commissioners Rhoads and Scattergood (not mentioned by Mr. Goldenweiser) of breaking up the reservations as quickly as possible consistent with justice to the Indian seems to be not only the more humane but the more practical course.

Apart from this chapter the book is amply documented with foot notes, and has three indices covering ten pages.

RECOLLECTIONS OF MY LIFE. *Memoirs of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. Volume VIII, Parts I and II.*

By Santiago Ramón y Cajal. Translated by E. Horne Craigie, assisted by Juan Cano. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia. $5.00. 9¼ x 6; xi + 638 + 21 plates; 1937 (paper).

So long as Nobel prizes are awarded, specialists in the fields of award are going to look askance at the judgment displayed in some of the bestowals. This is merely because the Swedish Committee, able, fair, and high-minded as it is, falls a little short of divine wisdom. But surely no award they ever made has been less criticised, and none deserved criticism less, than that to Santiago Ramón y Cajal. For he was an honest and a very great man, who single-handed worked out and *proved* beyond question or doubt the main features of the minute structure of the nervous system. That he did it under working conditions of extraordinary difficulty is of no real importance in assaying the real merits of the achievement, as he himself was aware. What is of importance is the simple fact that he did it. But over and above that great achievement stands his whole life. For he embodied and exemplified throughout every one of his many days the highest ideals of science, in conduct and in thought. He was simple not smart; he was honest not devious; he was a scholar not a politician; he held opinions because they were his not because they were soundly orthodox and likely to get him forward with important people; he was modest not self-seeking; he was forthright not diplomatic; he never defamed another that his own eminence might be the surer; he persistently and irrevocably sought the truth and found it. In short he had all the virtues that are but little esteemed in academic preferment.

The translator and the American Philosophical Society deserve great credit for making Cajal's autobiography available to readers of English. If any book can, in this day and age of the world be an inspiration to young biologists just starting a career, when things of the spirit are a bit out of fashion, this book will be. From a literary point of view the translation is pedestrian in spots, but it is accurate. We like it for just that reason. Ramón y Cajal was an accurate man.

REASONS FOR ANGER.

By Robert Briffault. Simon and Schuster, New York. $2.50. 8⅜ x 5⅝; 265; 1936.

It seems curious that the same man who can write articles such as *Family Sentiments*, and *Recent Anthropology*, should be the author of the bombast that appears under the label of *The Human Mind in Revolution*. In the last named essay occurs the following passage:

> The ideologies of morons, of well-to-do old ladies, of Babbitts, the pseudoscientific forgeries of a Professor Keller, or a Professor Osborne, the idiocies of a Millikan, to cite but some fragments of the falsification of all thought under the Dictatorship of the Bourgoisie, would be impossible and unthinkable under the Dictatorship of the Proletariat. They would be impossible, not because they would be censoriously repressed, but because they could not arise; they would be devoid of motivation. Such stupidities and mental contortions cannot occur in Russia.

We wonder. The moral, if one can be drawn, is perhaps that an anthropologist should stick to anthropology, and leave prophecy and novel writing to the literati.

Family Sentiments is an able attack upon the position of those anthropologists who are advocates of the theory of social origins from the paternal family. In Briffault's opinion, "The whole notion that social organization is founded upon kinship is erroneous. The truth is the exact reverse: kinship is founded upon social organization."

Lest any of our readers should be misled by the title of the book as to its content, we feel it our duty to say that a nicer perception of the precise use of language would have made it *Reasons for Mr. Briffault's Anger.*

MITLA: TOWN OF THE SOULS *and Other Zapoteco-Speaking Pueblos of Oaxaca, Mexico. By Elsie C. Parsons. University of Chicago Press, Chicago.* $4.00. 8 x 5½; xix + 590 + 51 plates + 3 folding maps; 1936.

Mitla is a town with a population of about 2000 located in southern Mexico, in the region of Oaxaca where ancient sepulchres and other ruins represent the remains of civilization of the Zapotecans conquered by the Aztecs. The author established herself in Mitla for the purpose of determining to what extent Spanish customs had superseded or been superimposed on the native culture and behavior patterns. In a discursive style, she describes the town, the kinds of people therein, their economic and their social life. She discusses in some detail their political activities, their religious behavior and their superstitions. The population she portrays is of a people predominantly of Indian stock, easy-going, tolerant, primarily interested in trading. The attempt to individualize or measure the Spanish influence on the *mores* of this people is rather unsuccessful. In part this is due to an apparent lack of sufficient knowledge of Spanish customs either past or present. This is especially noticeable in her emphasis on certain behavior patterns which, while they may be common to most southwestern Indians, are also common to Spanish and other peoples. While the author's conclusions presented in a very tentative fashion may be disregarded, the simplicity and fullness of her descriptions render this book an important contribution to this particular field of ethnology.

MEN OF MATHEMATICS. *By E. T. Bell. Simon and Schuster, New York.* $5.00. 9½ x 6½; xxi + 592 + 30 portraits; 1937.

These twenty-eight essays on the great mathematicians, starting with Zeno, and ending with Georg Cantor, make fascinating reading. Professor Bell is primarily interested in the mathematical principles developed by the men who have been included, and biographical matter is used to the extent that it is necessary to make clear the intellectual development of the mathematician whom he is discussing. However, we may add, it is biography of the first rank. In none of his earlier works has the learned and witty author, in our estimation, written with comparable brilliance.

The mathematicians have been selected for the importance of their contributions to the mathematical sciences. Legendre, admittedly a great mathematician, has been excluded because, in Bell's opinion, his work has been supplanted by that of Gauss, Abel, and Jacobi. While the nonmathematical reader will find a great deal to interest him, it is quite obvious that the volume is addressed to the mathematician, professional and amateur. It is a little difficult to see how a person who has never got beyond trigonometry can be in any way impressed by assertions to the effect that a given mathematical demonstration possesses great "beauty." And if the same reader, after having gone through the author's brief account of Riemannian surfaces, claims that he now understands why Riemann is so important a figure in the history of mathematics, he is pretty much of a humbug.

There is an index, and twenty-nine full page portraits.

WE, THE TIKOPIA. *A Sociological Study of Kinship in Primitive Polynesia.*

By Raymond Firth. Preface by Bronislaw Malinowski. American Book Co., New York. $6.00. 9¼ x 6; xxv + 605 + 25 plates + 6 folding charts; 1936.

"Tikopia lies in the extreme east of the British Solomon Islands Protectorate, and is inhabited by twelve hundred healthy and vigorous natives." Firth lived among these people for twelve months, the only white man on the island. Previous acquaintance with other Polynesian tongues reduced his language difficulties to a minimum, and he seems to have been quite successful in establishing himself as a trusted member of the community.

The Tikopia are a people without much interest in the elaborations of technology or of decorative art, but they have built up a complex system of social and religious activities, to which they attach great importance. They have a tradition of hospitality, a respect for birth and for wealth, a sensitiveness to public opinion, and a realistic attitude to social intercourse. This book is a sociological analysis of family life and kinship. The treatment is detailed, partly because in these days of rapid change of custom and break-down of ancient cultures it is essential to preserve as much as possible of the records of native institutions; partly because an anthropological study of kinship in primitive society needs much more than the summary account of the principles of organization which it usually receives.

Students of human biology will be grateful to Dr. Firth for his lucid and objective account of the customs of an interesting race. An excellent index has been provided.

RHYTHM FOR RAIN.

By John L. Nelson. Houghton Mifflin Co., Boston. $3.25. 8½ x 5½; xi + 272 + 54 plates; 1937.

The seventh child of Sikyanömsi was about to be born. It was to be a boy—the rebirth of the tiny son whose soul had been drawn back to the spirit world the preceding year and had been awaiting since then the opportunity to be born again.

Throughout the narrative woven around the birth, life and development of Sikyanömsi's twice born son the drama and ancient culture of the Hopi Indians is beautifully portrayed. The Kachinas, or Masked Gods, play a dominant rôle in the lives of these people, and the author in his descriptions of their ceremonies and dances has given a realistic picture of this form of American life. The title of the book is drawn from the dances performed during the period of the great drought to appease the angry rain gods. For nearly three years while this desert race became nearly annihilated, in dance upon dance they gave of their last measure of strength in a vain and heart-rending attempt to bring the rain. The book may well be classed as an epic.

Additional features include 30 pages of plates and a glossary of Hopi words.

PASCAL *The Life of Genius.*

By Morris Bishop. Williams & Wilkins Co., Baltimore, in Collaboration with Reynal and Hitchcock, New York. $3.50. 9¼ x 6; xi + 398; 1936.

Bishop's vivid portrayal of Pascal is characterized by insight and understanding. The figure of this genius emerges distinct, alive and without distortion. Although he himself is sympathetic, the author does not attempt to evoke either pity or unbounded enthusiasm for the great intellect that was unable to apply its distinctive powers of ratiocination to personal problems of behavior. Contrary to most biographers, the author believes that there was a woman in Pascal's life. At least, it seems that besides his sister, Pascal succeeded partially in directing another woman towards the life of asceticism and religious fervor that he so fanatically desired for himself and those he loved. The author gives a sound evaluation of the scientific and literary contributions of his subject; and in the description of the religious controversy in which Pascal was a protagonist, he is not swayed by his sympathies.

The human biologist, to whom the personality of Pascal is always of particular interest, will enjoy the elegant prose of this scholarly work.

REDISCOVERING ILLINOIS. *Archaeological Explorations in and around Fulton County. By Fay-Cooper Cole and Thorne Deuel. University of Chicago Press, Chicago.*

$2.00. 9½ x 6¾; xvi + 295 + 36 plates; 1937.

Scattered throughout Illinois are hillocks of various sizes constructed in by-gone times by the Mound Builders. Since 1925 the University of Chicago has been conducting an archaeological survey of these mounds. In 1930 the University of Illinois joined in the task. As the work progressed rather extensive excavations were undertaken. Six distinct cultures have been unearthed; two of these, the "Black Sand" and the "Red Ochre," were previously unknown. These and others are described.

Only the introductory chapter and the conclusions in this book are intended for the general reader. The remainder deals with "the more technical aspects of excavation and classification on which the general account is based." The text is interestingly illustrated with photographs and diagrams and is supplemented with numerous tables and appendices. There is also a glossary and an index.

GREAT FARMERS.

By J. A. Scott Watson and May E. Hobbs. Foreword by Right Hon. Walter E. Elliot. Selwyn and Blount, London. 12s. 6d. 8½ x 5¼; 287 + 16 plates; 1937.

This volume is made up of a series of quaint and enjoyable stories concerning the great men of agriculture in England during the third quarter of the 19th century. The period, almost co-extensive with Victoria's reign, is known as the "Golden Age of British Agriculture," and is noted for its "Kings of the Farm." The authors have given us something of the individuality, craftsmanship, and artistry of the farmers of this period, and have shown us why the period is worthy of study.

The early chapters of the book are devoted to the general land and cropping conditions of the British Isles. In the later chapters, we find something of the importance of live-stock raising to the welfare of the nation. The book is well written and beautifully illustrated by a number of half-tone and line prints. It will be of great interest to those who desire a more thorough knowledge of the past struggles and present status of agriculture in England.

LA SÉLECTION DU PERSONNEL. *Dans les Entreprises de Transport le Laboratoire du Travail du Réseau de l'État. Actualités Scientifiques et Industrielles, 376. Biologie du Travail et Biotypologie, I.*

By Pierre Lévy. Preface by M. R. Dautry. Hermann et Cie, Paris. 10 francs. 10 x 6½; 37; 1936 (paper).

The purpose of this monograph is to point out the need for routine physio-psychological examinations of applicants when selecting the personnel for industrial and commercial enterprises. The practical importance of such examinations is illustrated by observations made in the laboratory, recently established for this purpose, of the French State Railways. Here it has been noted that employees, regarded as able workers by their superiors or having a record of comparatively few accidents, obtained above average scores in the examination. In contrast, those who had had a relatively large number of accidents or whose working ability was considered poor also responded poorly to the various examinations. These consist of determination of certain somatological characteristics, tests of precision and coordination of movements, of reaction to sensory stimuli, of attention, memory and mechanical aptitude. A description of the examination technique is not given here.

COÖPERATION AND COMPETITION AMONG PRIMITIVE PEOPLES.

Edited by Margaret Mead. McGraw-Hill Book Co., New York. $4.00. 9 x 6. xii + 531; 1937.

One of the present-day trends in the field of sociological research is to trace the development of our very complex social order from its simplest beginnings among the primitive peoples through to its present day status. In this manner, an attempt has been made to find some of the fundamental principles underlying all social orders, and to show the ways and means of living and functioning of these

orders in relation to the individuals within them.

The present book is an intensive study of the way in which human relations are organized among thirteen different tribes along coöperative, competitive, or individualistic lines. The study, drawn from primitive materials, covers a broad range in the scale of social organizations. The volume as a unit, with the introduction and the interpretative statement, may be used as a basis for a systematic approach to the problems of culture and personality.

METHODOLOGY OF SOCIAL SCIENCE RESEARCH: *A Bibliography. Publications of the Bureau of Public Administration, University of California.*

By Dorothy C. Culver. University of California Press, Berkeley. $2.00. 9¼ x 6; x + 159; 1936.

This bibliography is an attempt to present a selected and annotated guide through the masses of new material of direct value to social science research which have been published in English since 1920. 1509 books and articles are here classified into nearly a hundred sections in which they are arranged alphabetically according to author. The table of contents consists of a classification outline of the subjects covered, and includes: Research Methodology, Selection of Problem, Sources of Material, Techniques of Analysis and Interpretation of Data, and Preparation of Manuscript. Two complete alphabetical indices cover both authors and subjects and refer to the separate items in the bibliography.

FIJI. *Their People, History and Commerce.*

By Nancy Walker. H. F. and G. Witherby, London. 7s. 6d. net. 8⅛ x 5½; 167 + 8 plates; 1936.

Impresssions received during a three months sojourn among the Fiji Islands and their peoples are recorded in this little book which makes pleasant reading. We find that towns which are often unprepossessing in appearance on approach, usually have their beauty spots and individual characteristics; that modes of travel are sometimes unique; that there is a racial problem developing with the steady growth of the Indian population which now nearly equals the native population; that life on the Islands is highly dependent upon its sugar trade. A brief review of Fijian history brings out the great contrast between the life of the present day on this Archipelago and that of less than a century ago.

THE OTHER HALF. *The Autobiography of a Tramp.*

By John Worby. Lee Furman, Inc., New York. $2.50. 8 x 5½; 307; 1937.

The author (who had little schooling) writes vividly and entertainingly of his experiences in "hoboing." Starting out at the age of sixteen after having escaped from a Canadian farm where he was placed by the authorities of an orphan asylum, he travelled far and wide in this country and England. Except for changes in spelling and punctuation and the omission of occasional passages the book is said to have been printed as it was written. It reveals much of the viewpoint and ethical standards of a man whose goal is to live well on his wits. With great candor the author describes at several points in great detail just what a lousy person he was. [This statement means exactly what it says.]

LIFE AND DEATH. *The Autobiography of a Surgeon.*

By Andrea Majocchi. Translated by Wallace Brockway. Knight Publications, New York. $2.75. 8¼ x 5½; 300; 1937.

The very first sections where Dr. Majocchi describes his extraordinary experiences as an interne at the Milan Obstetrical Station are the best, but from then on the trailing off in interest is rapid. A short visit to America, during which the hospitals of the East and Middle West were toured, left the author very unfavorably impressed by the amount of specialization among American surgeons. There is an uninteresting chapter about a Congress held in Tripoli, and others, scarcely better, on dueling, and on a pilgrimage to Lourdes.

Descriptions of operations, of course, take up a great deal of space. Some of Majocchi's superiors at the Milan Obstetrical Station would have delighted Balzac, and it is a pity that they rate so few lines here.

EUGENIK.

By Hermann Muckermann. Ferd. Dümmlers, Berlin. M. 4.40. 9 x 6⅛; viii + 173 + 8 plates; 1934.

The author, one of the leaders of the eugenic movement in Germany, presents a treatise of eugenics in the specific sense of the word. In this regard it is held to be the first of its kind. The material is based essentially on Muckermann's work as the Director of the Division of Eugenics in the Kaiser Wilhelm Institute for Anthropology, Human Genetics and Eugenics from 1927 to 1933. It is written in a clear, direct and interesting style, is illustrated, indexed and contains a biliography.

DIE VERSTÄDTERUNG. *Ihre Gefahren für Volk und Staat vom Standpunkte der Lebensforschung und der Gesellschaftswissenschaft.*

By H. F. K. Günther. B. G. Teubner, Leipzig and Berlin. RM. 1.60 (in Germany); RM. 1.20 (outside of Germany). 8¾ x 5½; 54; 1936 (paper).

This brochure is dedicated to Alfred Rosenburg, representative of the *Führer* and guardian of the education and growth of the national socialist movement. It is about the unquestionable superiority of the Nordics, with particular reference to the German Nordics. There is much musing, and dreaming and poetry mixed up with some racial anthropology.

STUDIES ON THE MUSCLES OF THE PELVIC APPENDAGE IN BIRDS.

By George E. Hudson. American Midland Naturalist. University of Notre Dame, Notre Dame, Ind. 50 cents. 9 x 6; 108; 1937 (paper).

Representatives of 16 orders of birds were used in this study. All of the muscles of the pelvic limbs were examined in 32 species and numerous other species were observed for specific points. The work was mainly by gross dissection but in a number of species celloidin cross sections were prepared and studied at various levels on the hind limb. Included in the study is a brief historical account of earlier work, a list of references and of abbreviations, and a group of 26 plates giving excellent illustrations of the main features of the investigation.

TRENDS IN DIFFERENT TYPES OF PUBLIC AND PRIVATE RELIEF IN URBAN AREAS, 1929–35. *U. S. Department of Labor, Children's Bureau, Bureau Publication No. 237.*

By Emma A. Winslow. Government Printing Office, Washington. 15 cents. 9¼ x 6; vi + 143; 1937 (paper).

The monthly expenditure from public and from private funds for different types of relief in 120 urban areas from January, 1929 to December, 1935 is given in tabular form. The various types of relief are analyzed separately for each of five large geographical subdivisions of the country, the classification being in accordance with the average monthly relief per family, and the average monthly number of families receiving aid.

A GRAPHIC SUMMARY OF FARM TAXATION. *U. S. Department of Agriculture, Miscellaneous Publication No. 262.*

By Donald Jackson. Government Printing Office, Washington. 5 cents. 9¼ x 5½; 17; 1937 (paper).

Certain of the more important facts about United States farm taxes are shown in the twenty-four graphs and maps of which this pamphlet largely consists. These include real estate taxes, farm property taxes, gasoline and automobile license taxes, net returns and tax delinquency for all the individual states.

WHERE ARE WE GOING?

By E. C. Harwood. American Institute for Economic Research, Cambridge, Mass. $1.00. 9 x 6; 88; 1937 (paper).

Such subjects as unemployment, the price of gold, the New Deal and World War inflations, future of the New Deal, wholesale prices, the influence of Maynard Keynes on our governmental policy of spending, etc. are discussed in a series of brief articles.

COMMERCIAL SHIPYARDS AND THE NAVY. *National Council of American Shipbuilders, New York.* 10 x 6¾; 105; 1937.

ZOÖLOGY

CHECK-LIST OF BIRDS OF THE WORLD. Volume III.

By James Lee Peters. Harvard University Press, Cambridge. $3.50. 9 x 5¾; xiii + 311; 1937.

THE DISTRIBUTION AND HABITS OF MADAGASCAR BIRDS. *Summary of the Field Notes of the Mission Zoologique Franco-Anglo-Américaine à Madagascar. Bulletin of the American Museum of Natural History, Volume LXXII, 1936, Article V.*

By A. L. Rand. American Museum of Natural History, New York. $2.50. 9¾ x 6¼; 357; 1937 (paper).

BIRDCRAFT. *A Field Book of Two Hundred Song, Game, and Water Birds. Ninth Edition.*

By Mabel O. Wright. Illustrations by Louis A. Fuertes. The Macmillan Co., New York and London. 89 cents. 7¾ x 5¼; xxiii + 317 + 80 plates; 1936.

THE ABC OF ATTRACTING BIRDS.

By Alvin M. Peterson. The Bruce Publishing Co., New York. $1.50. 7¾ x 5¼; v + 146; 1937.

Volume 3 of the *Check List* maintains the same high standard of previous volumes (Vol. 1 reviewed in Q. R. B. Vol. 7, No. 3). For those unacquainted with this series it should be said that it is designed to give a

"complete, authoritative, and up-to-date list of all known forms of recent birds, together with their ranges and all principal synonyms since the beginning of the century." The present volume "covers 142 genera and approximately 1675 forms, divided among the Sand-grouse, Pigeons (including Doves and Fruit Pigeons), and Parrots (including Lories and Macaws)."

For two years the members of the *Mission Zoologique Franco-Anglo-Américaine à Madagascar* were in the field making a collection of the birds and mammals of the island. The first part of their report on birds is devoted to "a consideration of the correlation between the habits and habitats, the distribution of the birds, a brief summary of the affinities of the avifauna, and a discussion of the migration and the breeding season in Madagascar"; the second, "to an account of the distribution and habits of the species, based on our observations." There are a number of illustrations but no index.

Birdcraft celebrates the twentieth anniversary of its existence by appearing in its ninth edition—a sufficient recommendation of its usefulness as a manual to bird lovers. The 80 full-page reproductions are from the paintings by Fuertes. The volume concludes with a key (23 pages) and indexes of both English and Latin names.

The last book in this group gives many practical hints on methods of attracting birds to become one's neighbors, and on how to establish an effective bird sanctuary.

WONDERS OF THE SEA: SHELLS. *15 Colour Plates Painted from Nature.*

By Paul A. Robert. With an Introduction by Julian Huxley and Introductory Text by Arnold Masarey. B. T. Batsford, Ltd., London. 5s. 6d. 11 x 8; 8 + 15 plates; 1937 (paper).

THE BEAUTY OF BUTTERFLIES. *Art and Nature in Colour.*

Introduction by Julian Huxley. Introductory text by Adolf Portmann. B. T. Batsford Ltd., London. 5s. 6d. net. 11 x 8; 8 + 12 plates; 1937 (paper).

The best possible way to write a review of these books would be to quote verbatim that part of Julian Huxley's introduction that is common to them both. The culmination of this is to be found in these words: "Here, certain facts of nature are presented. They are part of the raw material of science; however, they have been chosen, not for the scientific principles which they illustrate, but for the beauty which they embody." Why the scientist

should be concerned with the beauty of the objects he studies is adequately discussed.

Unfortunately, it costs money to produce such beautifully colored illustrations as comprise these books. Only a limited number of species may be figured without making the cost prohibitive. When we recall that authorities estimate the total number of described species of mollusca as between seventy and one hundred thousand, and that in contrast with this number only forty-five have been figured, we can realize that such books as these cannot go very far in imparting scientific knowledge. To do adequately would require about a thousand illustrations in each one, and then they would rank with Audobon's *Birds of North America*, which is the finest series of colored paintings of natural history objects that has yet appeared.

Each of these books contains a limited amount of explanatory printed matter, but the illustrations really constitute the books, and they cannot be too highly recommended to those who love the beautiful objects of nature and contemplate them with intelligence from the aesthetic standpoint.

KEYS TO THE FISHES OF WASHINGTON, OREGON AND CLOSELY ADJOINING REGIONS. *University of Washington Publications in Biology, Volume 2, Number 4.*

By Leonard P. Schultz. University of Washington Press, Seattle. 75 cents. 10 x 5; 126; 1936 (paper).

MARINE GAME FISHES *of the Pacific Coast from Alaska to the Equator. A Contribution from the Santa Barbara Museum of Natural History.*

By Lionel A. Walford, with paintings by Link Malmquist and photographs in natural color by Ralph Emerson. University of California Press, Berkeley. $5.00. 11 x 7¾; xxix + 205 + 69 plates; 1937.

The first of these publications is on both marine and fresh water fishes. In the keys, the statements are so arranged that one must consider alternative characters. With a little practice this system will be found most useful. The volume is illustrated and contains a glossary, and indices to common and scientific names occurring in the keys.

While the second of these books was written more for the angler it by no means falls into the "popular" class. The three authors aboard the *Haida* worked for two months studying, painting and photographing the fishes (mostly while still alive). Five new species were discovered and described. Detailed measurements and notes were made on at least five specimens of every species collected for anatomical variations. The volume contains 69 plates (36 colored), a glossary, keys, and index.

MY BIG GAME-HUNTING DIARY. *From India and the Himalayas.*

By Count Henrik Apponyi. Foreword by Viscount Halifax. Selwyn and Blount, London. 18s. net. 9 x 6; 254 + 157 plates; 1937.

In the foreword Count Apponyi expresses a naive suprise that his Indian hunting diary, written about his melodramatic and harrowing escapes, solely for his "dear mother" at home should ever be published. He shows himself to be a warm enthusiastic person who loves his friends above all else and prides himself on not being superstitious. Yet he is not fool enough to light "three on a match," and when he runs across home town friends in India, no matter how pleased he may have been, he realizes that their visit is limiting his killing, a thing unbearable to "all true sportsmen."

So eager is he to tell of the number of animals he shoots that in his boyish enthusiasm he sometimes quite naturally forgets to describe them, and his sportsman's love of hunting gives him little time to observe his prey before he shoots. When he does, however, he is not without appreciation for the beauty of the beast, and notes that the lovelier it is the stronger is his urge to kill. His 87 head of game present an enviable list of a winter's sport, and even exceeds the list of the celebrities he met, the palaces he stayed in, the banquets he attended, and the number of Rolls-Royces he rode in.

Numerous excellent photographs accompany the text and these not only in-

clude snaps of banquets and Maharajahs' automobiles, but also interesting ones of the scenery, old buildings, and native Tibetans.

THE PRACTICAL BEE GUIDE. *A Manual of Modern Beekeeping. Eighth Edition.*
By Rev. J. G. Digges. The Talbot Press, Dublin; Simpkin, Marshall, Ltd., London. 4s. 6d. (paper); 6s. (bound). 8½ x 5½; viii + 305; 1936.

Hives and other equipment involved in the handling of bees have become much more simplified in the United States than in many foreign countries. For this reason the bee keeper of the United States will have but little of practical value to gain from this treatise which is mainly devoted to descriptions of the more complicated apparatus and methods employed in the British Isles. While the book cannot be recommended for a beginner in this country, it is, however, interesting reading for one who is already acquainted with bees. Those chapters devoted to bee anatomy and bee behavior seem to be accurate and fairly inclusive. The book has an index but there is no bibliography.

MONOGRAPH OF THE GENUS EREBIA.
By B. C. S. Warren. British Museum (Natural History), London. £2. 10s. 11 x 7¼; vii + 407 + 104 plates; 1936.

This very fine monograph, on the brown Ringlet butterflies of the family Satyridae, gives testimony to the meticulous care with which its author made his study. His working basis was as follows:

(1) To supply definite proof, on an anatomical basis, of the real affinities of every hitherto described species, subspecies, form and aberration of *Erebia*; (2) to establish the correct use of every valid name, and to eliminate those that are only synonyms or homonyms; (3) to provide reliable data for the identification of species; (4) to treat the variation of each species comprehensively, but at the same time in as simple a manner as possible; (5) to give the distribution of each race accurately, but not in great detail, avoiding long distribution lists which would encumber the work without being of compensating value.

Included in the survey is a bibliography, a check list and an index of trivial names. The 104 plates give 1648 views of male and female species and subspecies of *Erebia*, of neuration and of male armatures.

SNAKES OF MARYLAND.
By Howard A. Kelly, Audrey W. Davis and H. C. Robertson. The Natural History Society of Maryland, Baltimore. 60 cents. 9¼ x 6½; 103; 1936 (paper).

This little book casts an interesting light upon one of the many and diverse interests of that amazing man, Dr. Howard A. Kelly. Interested in snakes from childhood, their collection and study has constituted a hobby throughout a long life. In *Snakes of Maryland* there are included descriptions of twenty-six non-venomous and two venomous species, with notes on habits, range throughout the United States and records of findings in Maryland. The text is supplemented with about thirty good clear photographs, eleven full page color plates beautifully painted by R. F. Deckert and with small diagrammatic drawings of the head and anal regions of several species.

LAND-IMPROVEMENT MEASURES IN RELATION TO A POSSIBLE CONTROL OF THE BEET LEAFHOPPER AND CURLY TOP. *U. S. Department of Agriculture, Circular No. 416.*
By R. L. Piemeisel and J. C. Chamberlin. Government Printing Office, Washington, 5 cents. 9¼ x 5½; 23; 1936 (paper).

This circular, based upon reconnaissance surveys of weed hosts and leafhoppers throughout southern Idaho, is an attempt to show that certain land-improvement measures would also help to control the beet leafhooper, vector of the curly top disease of sugar beets, beans, tomatoes and other cultivated crops.

As well-farmed lands and desert range in good condition do not produce economically significant numbers of this insect, the author believes that the leafhopper and curly top can be controlled if all lands, not continuously and well-farmed, can be restored and maintained as good desert range. The measures proposed for this accomplishment are similar to those

for land conservation, for in this the author sees not only increased forage and prevention of soil erosion, but also the control of the destructive beet leafhopper.

ECOLOGICAL STUDIES ON SELECTED MARINE INTERTIDAL COMMUNITIES OF MONTEREY BAY, CALIFORNIA.

By Willis G. Hewatt. American Midland Naturalist, University of Notre Dame, Notre Dame, Ind. 25 cents. 9 x 6; 46; 1937 (paper).

This report presents both qualitative and quantitative analyses resulting from an ecological investigation of the animal species occurring over a defined stretch of rocky littoral on the southern margin of Monterey Bay, California. The paper is of importance since it is the first to deal primarily with the sociological aspect of the littoral communities in this region. A definite zonation of the intertidal animals was found and various physical and biotic factors are considered in relation to this limited range.

There is included a systematic list of all the species (70) identified during the course of the work.

RESTLESS JUNGLE.

By Mary L. J. Akeley. Robert M. McBride and Co., New York. $3.00. 8¾ x 5¾; xiii + 313 + 32 plates; 1936.

Mrs. Akeley spent eight months in South Africa, principally in the Transvaal, Natal, and in Zululand. The expedition was undertaken for the purpose of studying the great game herds and the primitive people. The "hunting" was done entirely with camera.

Some of the most striking photographs are of Carl Akeley's groups in the African Hall of the American Museum of Natural History. A large portion of the book and perhaps the better portion, is description of native life, particularly that of Swaziland and Zululand.

MYTILUS. *L.M.B.C. Memoirs on Typical British Marine Plants and Animals. Edited by R. J. Daniel, XXXI.*

By Kathleen M. White. University Press of Liverpool, Liverpool. 9s. net. 9½ x 6; vii + 117 + 10 plates; 1937.

The series of L.M.B.C. Memoirs was begun in 1899 for the purpose of recording types of "common Irish Sea animals and plants of which no adequate account already exists in the textbooks." This thirty-first memoir deals with the mollusc *Mytilus*. A detailed description is given of the external characters and the internal systems. The history, habitat, food, bionomics, economic importance, fossil record, life history, and rate of growth are discussed. The text is illustrated with excellent plates, each of which is thoroughly explained, and there is an extensive bibliography.

ON JUNGLE TRAILS.

By Frank Buck and Ferrin Fraser. World Book Co., Yonkers-on-Hudson, N. Y. 96 cents. 7¾ x 5¼; vii + 280 + 29 plates; 1936.

This is the typical *Bring Them Back Alive* sort of book, with adventure as its primary interest and science its secondary. The volume presents eleven regular Frank Buck adventure stories, a list of animals he has brought to America, and a twenty-page outline of information concerning Asiatic mammals, birds and reptiles. Numerous photographs of animals and traps enhance its value as an adventure book.

LEÇONS DE ZOOLOGIE. *Prochordés. Amphioxus, Tuniciers (I. Ascidies). Actualités Scientifiques et Industrielles, 379.*

By M. Prenant. Hermann et Cie, Paris. 15 francs. 10 x 6¼; 77; 1936 (paper).

LEÇONS DE ZOOLOGIE. *Prochordés. Tuniciers (II. Pyrosomes), (III. Doliolides), (IV. Salpes), (V. Appendiculaires). Actualités Scientifiques et Industrielles, 380.*

By M. Prenant. Hermann et Cie, Paris. 12 francs. 10 x 6¼; 49; 1936 (paper).

These two numbers of a series of zoological texts, previous parts of which have been noticed from time to time in these columns, present an adequate, conventional survey of the development and structure of *Amphioxus* and five genera of

tunicates, as listed in the second title above. Both books are illustrated and selected bibliographies follow the discussion of each genus.

THE FURTIVE FOLK. *Feuds of Fur and Feather.*

By Dan Russell. Illustrated by Richard Ogle. Heath Cranton Ltd., London. 6s. net. 8½ x 5½; 186; 1937.

Although at times he tends toward the poetic and sentimental, Mr. Russell has given an excellent picture of the wild life of the English countryside. The stories of the hunter and the hunted, are simply told in this series of forty short sketches, and cleverly illustrated in black and white.

SPIDERS AND THEIR KIN. *Cornell Rural School Leaflet, Volume 30, Number 2.*

Prepared by E. Laurence Palmer. The New York State College of Agriculture at Cornell University, Ithaca. Free. 9 x 6; 32; 1936.

The present volume of the *Leaflets* is devoted to a résumé of the spiders and other forms closely related to them. The discussion, which is very elementary, includes the classification, behavior, and economic value of many of the common species. The photographs and drawings add considerably to its value as a teaching device.

DAS SAMMELN, KONSERVIEREN UND AUFSTELLEN VON WIRBELTIEREN. *Leitfaden für Sammler, Liebhaber und Fachleute nach neuzeitlichen Gesichtspunkten.*

By Gerhard Schröder. Verlagsbuchhandlung Paul Parey, Berlin. RM. 4.80 (less 25 per cent in foreign countries, except Switzerland and Palestine). 8½ x 5½; viii + 93 + 32 plates; 1936 (paper).

SAMMELN UND PRÄPARIEREN VON TIEREN. *Eine Anleitung zum Anlegen von zoologischen Sammlungen.*

By Georg Stehli. Franckh'sche Verlagshandlung, Stuttgart. RM. 2. 7⅞ x 5½; 95; 1936.

The first book listed is primarily intended for museum conservators and professional taxidermists. A great deal of emphasis is laid on stuffing each animal in a way that will make it look as nearly life like as possible and whenever possible to bring in some portrayal of its natural environment. More attention is given to birds and mammals than to any other vertebrates.

The second book is a simple instruction book for collectors, amply provided with illustrations. It is done with great thoroughness, though only the simpler preparations of forms of each species are described. The author begins with a discussion of the mammals and works backwards to the lower forms through birds, reptiles and snakes, fish and invertebrates.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXII Part 1, Numbers 1–6. Containing following articles: The Electric Discharge of the Electric Eel, Electrophorus electricus (Linnaeus),* by C. W. Coates, R. T. Cox and L. P. Granath; *The Templeton Crocker Expedition. II. Introduction, Itinerary, List of Stations, Nets and Dredges,* by William Beebe; *The Templeton Crocker Expedition. III. Brachygnathous Crabs from the Gulf of California and the West Coast of Lower California,* by Jocelyn Crane; *The Templeton Crocker Expedition, IV. Porcellanid Crabs from the Gulf of California,* by Steve A. Glassell; *The Templeton Crocker Expedition, V. A New Chrysomelid Beetle of the Genus Monoxia from Lower California,* by Doris H. Blake; *A New Species of Caulolatilus from Trinidad, British West Indies,* by William Beebe and John Tee-Van.

New York Zoological Society, New York. $1.60. 10½ x 7; 95 + 11 plates; 1937 (paper).

BOTANY

THE IDENTIFICATION OF TREES AND SHRUBS. *How to recognize without previous knowledge of botany wild or garden trees and shrubs*

native to the north temperate zone, with 2500 diagrams made by the author.

By F. K. Makins. E. P. Dutton and Co., New York. $4.00. 9¾ x 6; vii + 326; 1937.

FOREST TREES AND FOREST REGIONS OF THE UNITED STATES. *U. S. Department of Agriculture Miscellaneous Publication No. 217.*

By Wilbur R. Mattoon. Government Printing Office, Washington. 15 cents. 9¼ x 5¾; 54 + 14 plates; 1936 (paper).

GROWTH OF DOUGLAS FIR TREES OF KNOWN SEED SOURCE. *U. S. Department of Agriculture Technical Bulletin No. 537.*

By Thorton T. Munger and William G. Morris. Government Printing Office, Washington. 10 cents. 9¼ x 5⅞; 40; 1936 (paper).

The rules for the use of the key for the identification of trees and shrubs are easy to master. There are six pages of simple botanical terms alphabetically arranged, with which one must first have an acquaintance. The key itself is confined to one page where trees and shrubs with compound leaves or with normally simple leaves are sub-divided into leaf shapes and leaf arrangement. Having there found the most likely sounding group, one turns to the figures indicated and searches for the diagram most closely resembling his specimen. The name under the diagram may then be looked up in the index which refers to page of description. We should like to be able to recommend this key unqualifiedly because of its simplicity, but we have to admit we found some difficulty in checking up with specimens from the botanical gardens.

In the second publication all the tree species of continental United States are listed and classified according to the forest region in which they are found. Both common and scientific names are given with brief notes as to geographic range and distinguishing characteristics. Each list is preceded by a brief description of the forest region and a summary of the names of its principal trees. There are similar descriptions and summaries of the forests of Alaska, Puerto Rico and Hawaii.

Douglas fir being the outstanding tree of the commercial timber zone of western Washington and western Oregon, it was selected for experimental use in testing and comparing the growth made from seeds which had matured under one set of conditions (involving age, health, growing space and quality of site of the parent trees and the altitude at which they were found), when they were grown under other, varying conditions but still within the principal range of the Pacific coast form of Douglas fir. This report contains a description of the experimental procedures—which were initiated in 1912 and 1913—and includes an analysis of the results to date.

MANUAL OF THE GRASSES OF THE WEST INDIES. *U. S. Department of Agriculture, Miscellaneous Publication No. 243.*

By A. S. Hitchcock. Government Printing Office, Washington. $1.25. 9 x 5¾; 439; 1936.

BRITISH GRASSES *and Their Employment in Agriculture. Third Edition.*

By S. F. Armstrong. The University Press, Cambridge; The Macmillan Co., New York. $5.25. 8½ x 5½; ix + 350; 1937.

These books both describe various grasses in considerable detail. In the volume on the grasses of the West Indies Hitchcock devotes only a couple of pages to a very brief and general summary of the economic uses of these plants. The rest of the volume is comprised entirely of scientific descriptions, in accordance with the International Rules of Botanical Nomenclature, of all the known species of West Indian grasses. On the other hand, the author of the volume on British grasses has produced a completely practical book written primarily for students of agriculture. The first part, the botanical section, deals with the morphology, general biology, grouping according to various characters, and botanical description of species. All the native species are described but more space is given to those of greater abundance or of greater economic importance. The second part, the agricultural section, deals with the economic, agricultural, and commercial aspects of grass crops and also includes a chapter on lawns and greens. Both books are well indexed and illustrated.

OKLAHOMA FLORA.

By Thomas R. Stemen and W. Stanley Myers. Harlow Publishing Co., Oklahoma City. $6.00. 7½ x 5¼; xxix + 706; 1937.

This fine volume is a key for the classification and identification of all the flora of Oklahoma, exclusive of the grasses, sedges, and rushes. The book was prepared primarily for use in Oklahoma, but it describes species common to Kansas, Arkansas, and Texas, and so may be used in identifying the flora of these neighboring states.

The volume includes a preface, a table of contents, the analytical key to families, appendices of (a) Wild Flowers that should be protected; (b) Hay-fever plants; (c) Poisonous plants; (d) Drug producing plants; (e) Native shore and water plants useful as food and shelter for pond fish, frogs, tadpoles, crayfish, wild ducks, etc.; (f) Edible plants; a glossary; an index of scientific names; and an index of English names.

Many species are illustrated with drawings of roots, stems, leaves, flowers, and fruit, or by a drawing of the entire plant. The *Flora* will be of service to students of biology and botany as well as to the nature loving public.

WINE GRAPES. *Their Selection, Cultivation and Enjoyment.*

By Philip M. Wagner. Harcourt, Brace and Co., New York. $2.50: 8 x 5¼; xii + 298 + 8 plates; 1937.

There are chapters on "What the Vine Is," "How the Vine Grows," "Climate and Soil," "Propagating the Vine," "Establishing the Vineyard," "Cultivation and Harvesting," etc. The various grape-growing districts scattered over the United States are described, and the vine varieties that are best supported by each are indicated. The long list of grape varieties, giving under each the type of wine that the grape produces, kind of soil to which it is best adapted, pruning, disease prevention, etc. should be of value to anyone interested in vine culture. The language is non-technical. It is worth noting that the grapes which produce the best wines are difficult, if not impossible, to obtain outside California. Some Californians, at any rate, have access to excellent domestic wines—but that does not make the rest of us feel any more cheerful.

The book is well illustrated and indexed.

METHODS FOR THE MEASUREMENT OF CERTAIN CHARACTER PROPERTIES OF RAW COTTON. *U. S. Department of Agriculture, Technical Bulletin No. 545.*

By Howard B. Richardson, T. L. W. Bailey, Jr., and Carl M. Conrad. Government Printing Office, Washington. 15 cents. 9⅛ x 5¾; 77; 1937 (paper).

This paper summarizes the latest experimental work on determining the strength of raw cotton fibers. The authors have pointed out the errors and difficulties that were present in the older methods of measuring character properties, and have eliminated, or corrected them in such a way that a more efficient technique has evolved. Methods for calculating the biometric constants describing the different characteristics of raw cotton have been given in detail. The work is well illustrated by tables, charts, and graphs, as well as photographs of apparatus, and micro-photographs of the different types of cotton fibers. A bibliography of 70 papers, and an appendix of tables and formulae pertinent to the discussions are valuable additions to the paper.

BUD SELECTION IN EUREKA AND LISBON LEMONS AND PROGENY TESTS OF BUD VARIATIONS. *U. S. Department of Agriculture, Technical Bulletin No. 531.*

By A. D. Shamel, C. S. Pomeroy and R. E. Caryl. Government Printing Office, Washington. 10 cents. 9¼ x 5⅞; 44; 1936 (paper).

According to the authors, practically all of the lemons grown commercially in the United States have been developed from two original strains: the Lisbon and the Eureka. Apparently, bud variations of individual fruits, limbs, and entire trees are fairly common. In this pamphlet some of these variations, the frequency of

their occurrence, their significance, and their relative inherent stability are discussed. Data have been drawn from the Citrus Experiment Station orchard and from a commercial orchard, and the importance of the results to the industry is indicated.

ZUR SOZIOLOGIE DER ISOËTETALIA. *Pflanzengeographische Kommission der Schweizerischen Naturforschenden Gesellschaft. Beiträge zur geobotanischen Landesaufnahme der Schweiz, Heft 20.*

By Max Moor. Hans Huber, Bern. 6.50 francs. 9 x 6¼; 148 + 7 plates + 2 folding charts; 1936 (paper).

Five species of *Nanocyperion* and one of *Isoetion* are treated in this book. The former group is found in Central Europe, the latter in the Mediterranean regions of southern Spain, France, the islands west of Italy and northern Africa. Information for each species includes general and floral characteristics, structure, periodicity, ecology and geographic distribution. A six-page bibliography is included.

MORPHOLOGY

HISTOIRE DES ORIGINES DE LA THÉORIE CELLULAIRE. *Actualités Scientifiques et Industrielles, 328. Exposés d'Histoire et Philosophie des Sciences, III.*

By Marc Klein. Hermann et Cie, Paris. 15 francs. 10 x 6½; 72; 1936 (paper).

This short but informative work relates the history of the cellular theory from Hooke's *Micrographia* through Max Schultze's famous article *Ueber Muskelkörperchen*, with a brief mention of more recent writings up to about 1904. It describes clearly and succinctly the chief courses of thought in the development of this subject, and mentions the more important disputes and points of disagreement. It is to be hoped that this monograph will be translated into English, for it would be of considerable value to undergraduate students as collateral reading in even rather elementary college courses in biology.

PHYSIOLOGY AND PATHOLOGY

WHO GAVE THE WORLD SYPHILIS? *The Haitian Myth.*

By Richard C. Holcomb. Introduction by C. S. Butler. Froben Press, New York. $3.00. 9 x 6; 189; 1937.

The intention of the author is to demonstrate that there is no factual foundation to the theory which considers Haiti as the place of origin of syphilis. The greater part of this monograph is dedicated to a chapter by chapter summary of a book by Ruiz Diaz de Isla, the Spanish surgeon whose writings are generally accepted as *prima facies* evidence of the importation of syphilis from Haiti. Ruiz Diaz stated that in Barcelona, in 1493, he had treated for syphilis a pilot from Palos. This statement, according to Holcomb, was later cited to read that Ruiz Diaz treated one of the Pinzon brothers after the return from the first voyage made by Columbus. This was proof that the first case of syphilis had come from America. Very neatly, Holcomb shows that it is highly improbable if not impossible that the pilots treated by Ruiz Diaz and Pinzon were one and the same person. Similarly, when all the so-called evidence is closely scrutinized, the numerous internal contradictions are clearly apparent and indicate the weakness of the theory. Holcomb comments that probably, in the years that followed the discovery of America, a number of misstatements were deliberately made by physicians and other authorities as part of a sales campaign for the introduction of Haitian Holy Wood or Guaiacum, supposedly a specific for the disease. Moreover, Holcomb notes that description of syphilis as given by Ruiz Diaz is not consistent with the disease as known now, nor was that writer certain whether it was a new disease, or Pliny's *Mentagra*.

The book is interesting but suffers from disorder in presentation and attempts at irony and sarcasm which miss the mark. These and other minor defects, that could have been avoided, detract somewhat from the value of this book, which otherwise demonstrates unusual scholarship and clear thinking.

THE PATIENT AND THE WEATHER. *Volume IV, Part I. Organic Disease. Cardio-Vascular-Renal Disease, including a Chapter on Experimental Endocarditis by Alexander J. Nedzel.*

By William F. Petersen and Margaret E. Milliken. Edward Bros., Ann Arbor, Mich. $10.00. 10¾ x 8¼; xxxiv + 663 + 19 plates; 1937.

Petersen continues to develop the Hippocratic thesis regarding the relationship between atmospheric changes and physiologic alterations (*cf.* reviews of earlier volumes in Q.R.B., Vol. 10, No. 3., Vol. 11, No. 2, Vol. 11, No. 4). In this volume he discusses especially the influence of meteorologic shifts on the onset and exacerbation of clinical episodes in cases of cardiovascular-renal diseases, although other conditions are also surveyed *en passant*. The evidence is furnished by clinical histories of patients more or less observed by the author as well as cases reported in the literature. Moreover the author presents statistics of death from cardiovascular-renal diseases, alleged to show that days having the most frequent deaths followed or were near periods of significant weather changes.

As has been noted in previous reviews, the evidence so far presented is not adequate to permit either agreement or disagreement with the author's viewpoint. The weather is in such a state of fluctuation that it is not difficult, to find periods in which, for example, some clinical condition does not precede, follow or coincide with a polar episode. Until definitive evidence is presented, one may only admire the author's eruditions, philosophical viewpoint and his broad general knowledge of physiology, pathology and clinical medicine, but deplore the lack of a refined analytic technique in this investigation.

ANLEITUNG ZUR KONSTITUTIONSDIAGNOSTIK BEI KINDLICHER TUBERKULOSE. (*An Hand von praktischen Beispielen.*) *Praktische Tuberkulose-Bücherei, Beihefte des deutschen Tuberkulose-Blattes, Heft 17.*

By Kurt Klare. Georg Thieme, Leipzig. RM. 4.80. 9⅝ x 6⅝; 56; 1937 (paper).

Twenty years experience with childhood tuberculosis leads the author to conclude that this condition is essentially a manifestation of an innate constitutional anomaly. He notes that in most instances tuberculous children will also show the characteristics of a so-called exudative-lymphatic constitution. So he believes that when the disease appears in such children it should be regarded simply as another expression of their particular diathesis. In these cases prognosis is good. Instead, when tuberculosis appears in children who possess constitutional characteristics other than these, prognosis is only fair.

To illustrate his point, the author summarizes the clinical and laboratory observations made on 16 tuberculous children. Notwithstanding the apparent severity of the lesion in some cases, the children with an exudative-lymphatic constitution turn out rather well, while those with other constitutional characteristics do not.

The author's thesis agrees with the general theory advanced since the time of Hippocrates by physicians interested in the individual constitution. However, lack of a precise definition of the constitutional characteristics and of sufficient evidence does not permit definite conclusions on the subject.

SPORT UND ARBEITSSCHÄDEN. *Eine Zusammenfassung klinischer Beobachtungen und wissenschaftlicher Erkenntnisse zur Biologie der Arbeit und Pathologie der Funktion.*

By Wilhelm Baetzner. Georg Thieme, Leipzig. RM. 8 (paper); RM. 9.60 (cloth). 10⅛ x 7; 136; 1936.

The author of this book is in somewhat of a dilemma to reconcile his findings with the national socialist movement and all the strain it puts on the young through enforced gymnastics, hikes, *Arbeitsdienst*, etc. So at short intervals through the book he is at pains to state that his findings are not a criticism of the youth movement in Germany, and that sensible exercise is always excellent for building good bodies. In general except for a short history of the gymnastics, the first section of the book is made up of truisms and generalizations. By far the greater part of the book is devoted to descriptions of

pathologic changes in the various organ systems. The author shows several examples from contestants in the 1936 Olympic games, particularly, of the bone changes in the knees, femur, tibia, and foot bones, arising out of jumping and sprinting. There are numerous X-ray pictures showing these effects.

On the whole the book is excellent, and contains a lot of useful information for the medical man. There is a bibliography and index.

PHYSIOLOGICAL HYGIENE.

By Cleveland P. Hickman. Prentice-Hall Co., New York. $3.25. 8 x 5½; xxvi + 493; 1937.

This is another book devised to help in the teaching of hygiene and physiology to college students. The author's experiences convinced him that students want to understand the underlying reasons for "cultivating good health behavior. In other words, they want the physiological background of why they should form a particular habit." Doubtless to a large extent Hickman is right. Most of us would hate to put ourselves out and change our habits of life unless we were given good reasons for doing so.

It is hard to tell even after careful inspection of a book like this whether or not it has distinct advantages over other books which are already on the market. Our impression is that it is better than a good many. The illustrations showing the pelvis are not anatomically correct and in a later edition should be done over.

DICTIONNAIRE DES BACTÉRIES PATHOGÈNES *pour l'homme, les animaux et les plantes.*

By P. Hauduroy, G. Ehringer, A. Urbain, G. Guillot and J. Magrou. Masson et Cie., Paris. 120 francs (paper); 140 francs (bound). 9½ x 6¼; 598; 1937.

In this work, the authors have made no attempt to give an exhaustive list of the pathogenic bacteria, but have given, in a brief and precise manner, some of the characteristics of the more common bacteria of plants and animals, as well as of man. Some 650 bacteria have been listed alphabetically by order, family, genera, and species, with a brief summary of the morphology, culture methods, biochemical and biological properties of each. The important literature concerning the different bacteria is listed in the foot-notes. The book is concluded with a number of tables describing the characteristics of bacteria, a complete index, and a 50-page list of *Ouvrages de Médecine Récents*. The volume will, no doubt, be of value to the medical student and to the student of bacteriology.

ENTSTEHUNG UND BIOLOGISCHE BEKÄMPFUNG TYPISCHER INFEKTIONSKRANKHEITEN. *Vorlesungen auf Grund der Ergebnisse experimenteller Untersuchungen. Erste Folge.*

By Richard Bieling. Johann Ambrosius Barth, Leipzig. RM. 6.60. 9¼ x 6¼; vi + 119; 1937 (paper).

This little book is a condensation of a lecture course given by the author. Three infectious diseases, diphtheria, pneumonia and poliomyelitis are discussed in detail. Some further publications by the same author covering other diseases will appear sometime during the next two years.

The purpose of the lectures was to portray as exactly as possible the biological picture or significance of each stage of the development of each of the diseases from the moment of the entering of the infecting agent into the body until the death of the individual or the death of the infecting organism. Active and passive immunity for each of the three diseases is discussed in detail. There is a detailed bibliography for each disease.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 463. Gehörsinn, Hautsinne, Kraftsinn (Muskelsinne), Geschmack- und Geruchsinn, Statischer Sinn, Stimme und Sprache.* Containing following articles: *Die Herstellung eines praktisch vollkommen geräuschlosen Raumes*, by G. van Rijnberk; *Verfahren zur Prüfung der Leistungen des Geruchssinnes; Neue Verfahren zur Prüfung der Leistungen des Geschmacks-*

sinnes; Neue Verfahren zur Erforschung der Leistungen des Druck-, Temperatur-, Schmerz- und Kraftsinnes; all by Emil von Skramlik. *Urban und Schwarzenberg, Berlin.* RM. 9. 10 x 7; 160; 1937 (paper).

The first paper in this number described the construction of the new sound-proof room in the Physiology Laboratory at Amsterdam. This description is very thorough, including as it does the construction of screen doors to allow fresh air to enter but to keep out the mice, and of the equipment (telephone, buzzer, axe, etc.) for getting assistance when the experimenter has locked himself in or the doors stick. The other papers discuss sensory tests conducted at Jena. The last paper is not complete. All are illustrated and annotated.

AUTONOMIC NEURO-EFFECTOR SYSTEMS.

By Walter B. Cannon and Arturo Rosenblueth. The Macmillan Co., New York. $4.00. 8½ x 5⅛; xiv + 229; 1937.

In the preface the authors state:

> Our central interest in writing this monograph has been to render an organized account of recently acquired evidence regarding the chemical step which intervenes between the nerve impulse and the effector in the functioning of the autonomic nervous system. A wider range of interest, however, is implied in the title, *Autonomic Neuro-Effector Systems.* This title was chosen because we have considered not only the processes in the junctional region but also those occurring in the effectors themselves.

The volume is illustrated, contains an excellent list of references and is indexed.

TOD UND TODESURSACHEN UNTER DEN BERLINER JUDEN.

By Franz Goldmann and Georg Wolff. Foreword by E. Seligmann. Reichsvertretung der Juden in Deutschland, Berlin-Charlottenburg. RM. 1. 9¼ x 6⅛; viii + 82; 1937 (paper).

This investigation of causes of death among Berlin Jews covers 14,083 cases during the years 1924–26 and 1932–34. These cases were compared with a group of Aryans for the same period and considerable differences were found. Infant mortality, tuberculosis, and to a lesser extent cancer and apoplexy showed much lower death rates among Jews than Aryans. On the other hand diabetes, circulatory diseases, particularly arteriosclerosis and to some extent suicide take a higher death toll among the Berlin Jews. Suicide has risen 50 per cent for the years 1932–34. There are twelve tables showing causes of death by different classifications, age groups, sex, etc.

MATERIA MEDICA, TOXICOLOGY AND PHARMACOGNOSY.

By William Mansfield. C. V. Mosby Co., St. Louis. $6.75. 9 x 6; 707; 1937.

This book is attractively gotten up and well illustrated with photographs. It is not designed for reading because it is so brief and schematic. It is a textbook and a work of reference. With few exceptions each drug is given but one page of description. At the end there are about 120 pages on the various poisons, the symptoms produced and the treatment indicated. The book is obviously designed for the training of druggists and medical students.

TRAVAUX PRATIQUES DE BACTÉRIOLOGIE.

By Henri Bonnet and Armand Nevot. Preface by Robert Debré. Masson et Cie., Paris. 38 francs. 7⅛ x 5½; vi + 178 + 6 plates; 1936 (paper).

For its size this is the most promising looking and the best illustrated book on bacteriology that we have seen. There are many excellent colored plates showing stained bacteria, cultures, and the Wassermann and other reactions. Translated into English this little book would probably be much appreciated by American medical students.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Leiferung 462. Quantitative Stoffwechseluntersuchungen (Ergänzung zu Abt. IV, Teil 13).* Containing following articles: *Ein Gaswechselapparat für Kleintiere,* by K. Junkmann; *Methodik der thermoelektrischen Hauttemperaturmessung,* by H.

Pfleiderer and K. Büttner; *Elektrische Gaswechselschreibung an Tier und Mensch*, by H. Rein; *Neuere Methoden der Spirographie und Spirometrie*, by M. Schneider and W. Schoedel; *Neue Geräte für direkte und indirekte Kalorimetrie*, by W. Bothe and H. Wollschitt.

Urban und Schwarzenberg, Berlin. RM. 14. 10 x 7; 236; 1937 (paper).

Discussions of new methods for measuring and recording gaseous metabolism, skin temperature and body temperature. Detailed descriptions and illustrations of apparatus and examples of their application are presented.

BIOCHEMISTRY

La Température Critique du Sérum. *I Viscosité et Phénomènes Optiques. Actualités Scientifiques et Industrielles, 401. Biophysique Moléculaire, I.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 18 francs. 10 x 6½; 84; 1936 (paper).

La Température Critique du Sérum. *II Phénomènes Optiques et Phénomènes Ioniques. Actualités Scientifiques et Industrielles, 402. Biophysique Moléculaire, II.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 20 francs. 10 x 6½; 104; 1936 (paper).

La Température Critique du Sérum. *III Fixation d'Ether-Tension Interfaciale et Spectre d'Absorption Ultra-Violet. Actualités Scientifiques et Industrielles, 403. Biophysique Moléculaire, III.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 10 francs. 10 x 6½; 41; 1936 (paper).

Until the present time, the problems of blood and of blood serum have been mainly studied only in connection with the general physiological or pathological condition of an organism. It has been the attempt of the author to point out the value of the study of serum apart from physiology and pathology, and to encourage further investigation along this line.

In this series of three small volumes du Noüy has given us some new facts, based on experimentation and observation, which are intended to clear up some of the fundamental problems on the mechanism and reactions of immunity. By the application of certain statistical and mathematical principles, a norm for serum has been determined. A comparison of the characteristics of "normal" serum with the characteristics of similar sera at different temperatures has brought to light many interesting facts. The work marks a step forward in the field of immunology.

The Little Things in Life. *The Vitamins, Hormones, and Other Minute Essentials for Health.*

By Barnett Sure. D. Appleton-Century Co., New York. $2.50. 8 x 5½; xi + 340; 1937.

This discussion of the rôle of vitamins, hormones and minerals in health and disease is one of a series of books intended to familiarize laymen with recent advances in medicine. Sure has abstracted a large mass of technical literature but, unfortunately, he has left out the details that a physician would like to know and he has put in too many research findings for most laymen to digest. It is far from certain that these sketchy notes on the very latest vitamin and hormone research will be any protection to the layman against the quackery that the author deplores.

Home economics students will be able to use it as a reference book advantageously. The best chapters deal with the rôle of vitamins during pregnancy and the nursing period, and with malnutrition in children. There is a convenient tabulation of the vitamin content (including vitamins E and G) of two hundred odd foodstuffs. Although the preface contains the promise that the book is written in "non-technical language" the author found it necessary to include a five page glossary of the technical terms he did use. There are both author and subject indexes.

La Coacervation et Son Importance en Biologie. *Tome I Généralites et Coacervats Complexes. Actualités Scientifiques et Industrielles, 397. Exposés de Biologie, VI.*

By H. G. B. de Jong. French version by N. I. Joukovsky and Jean P. E. Duclaux. Hermann et Cie, Paris. 12 francs. 10 x 6½; 54; 1936 (paper).

LA COACERVATION ET SON IMPORTANCE EN BIOLOGIE. *Tome II Coacervats Auto-Complexes. Actualités Scientifiques et Industrielles, 398. Exposés de Biologie, VII.*

By H. G. B. de Jong. French version by N. I. Joukovsky and Jean P. E. Duclaux. Hermann et Cie, Paris. 15 francs. 10 x 6½; 66; 1936 (paper).

In 1929 the author coined the word *coazervation* for a phenomenon in colloidal systems which resembles the separation of phases. It is applied in cases where the disperse phase appears in a concentrated and colloid-rich fluid and in which the factors of capillary electric charge and solvation are involved. He distinguishes between simple coazervation in which only one type of colloidal particle is concerned, and complex wherein two or more kinds are involved. It is with the latter types that these two volumes are particularly devoted as these are the ones most often found in living matter. After a discussion of general principles, specific applications are described and the differences between synthetic models and the living cell brought out. This is an important, but very technical, contribution in a highly specialized branch of physical biology. The bibliography is confined to works of the author and his co-workers.

ACTION DES LIPOÏDES SUR LES PHÉNOMÈNES DE LA LYSE. (*Saponine, Bactériophage, Rayons X*). *Actualités Scientifiques et Industrielles, 404. Biophysique Moléculaire, IV.*

By Baruch S. Levin. Hermann et Cie, Paris. 15 francs. 10 x 6½; 83; 1936 (paper).

This is the report of a series of experiments made to determine the influence of lipoids in cytolysis and hemolysis produced by chemical and physical means. The experiments were conducted by the author and several collaborators and had for their main purpose to extend previous observations. In agreement with these, it was found that lecithin and cholesterin inhibited to some extent the cytolytic action of saponin and x-rays on paramecium. In other experiments, the presence of these lipoids increased resistance to the cytolytic action of bacteriophages. Experiments regarding hemolysis gave similar results. Here again, the presence of either lecithin or cholesterin reduced the lytic action of saponin and of x-rays. The results of these experiments seem to be definitive, but the experimental procedures are not described as completely as is desirable.

THE METABOLISM OF LIVING TISSUES.

By Eric Holmes. The University Press, Cambridge; The Macmillan Co., New York. $2.25. 7¼ x 5; x + 235; 1937.

The author's purpose in writing this book is to present a survey, biological more than purely chemical, of the present range of knowledge in dynamic biochemistry. The volume is based on a great volume of sound experimentation and observation. The emphasis is on the activity of living tissues rather than on the substances which their actions transform. This is an unusual treatment of the subject, but one that reflects a modern tendency in the methods of research and teaching in biochemistry. The book has been prepared not only for biochemists, but for those who are beginning the study or are otherwise interested in recent scientific developments.

STOFF UND LEBEN. *Bios, Band VI.*

By Hans Schmalfuss. Johann Ambrosius Barth, Leipzig. RM. 13.60. 9¼ x 6½; xvi + 203; 1937 (paper).

This is a general treatise on the problems of biochemistry discussed from the biological viewpoint. In order to reach the attention of readers of various scientific disciplines and of various degrees of education, the author endeavors to write in a simple and generally intelligible style. To this end he shuns the use of technical scientific nomenclature, employing instead expressions of German coinage: e.g., *Stoffkunde* for *Chemie*, *Lebenskunde* for *Biologie*, *Erb* for *Gen*, *Ergänzungsnährstoff* for *Vitamin*, *Festwert* for *Konstante*, *Kerbtier* for

Inseks, Regler for *Katalysator, Wirkstoff* for *Hormon*, etc. etc. Thus the language may perhaps advance the *anschauliche Denken* for German readers, but for the foreign student it will make the perusal of the book more difficult. Fortunately the author furnishes a list of the employed *Verdeutschungen*. The work is annotated and supplied with illustrations, index of names and a topical index.

TRAITÉ DE CHIMIE ORGANIQUE. *Tome IV. Noyau Benzénique. Carbures Benzéniques. Dérivés Halogénés. Acides Sulfiniques, Sulfiniques ou Séléniques. Acides Sulfoniques. Sulfones. Dérivés Nitrés. Nitrohalogénés. Nitrosés. Dérivés Nitrosulfiniques, Nitrosulfoniques, Nitrohalogénés. Groupe du Biphényle et Polyphényl Benzènes. Dérivés di-, tri- et Polyphénylés du Méthane et de l'Éthane. Polyaryl Propanes et Homologues Supérieurs. Carbures Polyaryliques non Saturés. Arylcyclohexanes Divers. Industrie des Chlorobenzènes et Chlorotoluènes. Pétroles. Industries Dérivées des Combustibles Solides.*

By P. Amagat, P. Baud, L. Bert, L. Boisselet, Cl. Duval, J. Doeuvre, R. Heilmann, G. Hugel, J. Lichtenberger, M. Louis, L. Piaux, T. Salomon, F. Swarts, E. Vellinger, H. Weiss. Published under the direction of V. Grignard and Paul Baud. Masson et Cie, Paris. 220 francs (paper); 240 francs (cloth); 10 x 6½; xxvii + 828; 1936.

This is the fourth volume of a comprehensive work on organic chemistry which when complete, will include fifteen volumes. The scope of this volume is as indicated in the subtitle. The preceding volumes have previously been noticed in these columns in Vol. 11: Volume I in Number 1, Volume II in Number 4, and Volume III in Number 2.

OUT OF THE TEST TUBE. *Second Edition, Revised and Expanded.*

By Harry N. Holmes. Emerson Books, Inc., New York. $3.00. 9¼ x 6¼; 301; 1937.

Out of the Test Tube is a popular discussion of chemistry and of many of the chemical conquests which have profoundly influenced the life of the modern age and it quite definitely stands out as a leader among the books of popularized science. Dr. Holmes has not been content with merely calling attention to the many "wonders" which have been produced through the medium of this branch of science, but has given sufficient background so that the intelligent layman may gain an appreciation of the methods underlying chemical research and reasoning. The list of subjects given consideration in this new edition is a long one as is indicated by its four page index.

The book is recommended to all who have a keen interest in the affairs of life.

CHEMIE DER INKRETE *und ihre wichtigsten Darstellungsmethoden. Zwanglose Abhandlungen aus dem Gebiete der Inneren Sekretion Band I.*

By Kurt Maurer. Johann Ambrosius Barth, Leipzig. RM. 7.20. 9¼ x 6½; vi + 67; 1937 (paper).

This inaugural volume of a series of endocrinological works as related to medical practice and theory is a good, concise exposition of what is known of the chemistry of the internal secretions. The material includes descriptive material on the chemical structure and action, and directions for tests. It is annotated but not indexed.

PROCEEDINGS OF THE SOCIETY OF BIOLOGICAL CHEMISTS, (INDIA). *Volume 1 (January 1936–November 1936).*

Society of Biological Chemists, Indian Institute of Science, Hebbal P. O., Bangalore. Re. 1 (postage extra). 8⅜ x 5½; ii + 45; 1936 (paper).

SEX

HISTORY OF MODERN MORALS.

By Max Hodann. Translated by Stella Browne. William Heinemann (Medical Books), London. 12s. 6d. net. 8½ x 5½; xv + 338; 1937.

The title of this book is not a true indication of its contents. First of all, only sex taboos are considered, and secondly, it is essentially an apology for and a eulogy of sexology. In the first chapter, the author discusses the theory of evolution and the development of genetics, embryology and endocrinology. Here he notes that the public reaction against sexology is similar to that which occurred with respect to the theory of evolution. There follow chapters on homosexualism, prostitution and the campaign to eradicate syphilis, the development of sexology, birth control, legalized abortion, sex education, psychoanalysis, woman suffrage. For each subject, the author demonstrates the strength of the existing taboos and how biological reality is disregarded in order to enforce the mores.

It is a book shrewdly prepared to appeal to the emotions of intellectual liberals whose political ideas are slightly tinged with red. The battles of the sexologists, their objectives and the value of their work are overly dramatized and somewhat exaggerated. Each chapter is furnished with a fairly complete bibliography, and a list of the dates of occurrence of significant events for sexology. Notwithstanding its aim, which is that of propaganda, this publication is one of the best general discussions of the subject to appear in recent years.

PRACTICAL BIRTH CONTROL BY NATURE'S METHOD. *With an Appendix on How to Promote Conception.*

By George R. Scott. Cassell and Co., London. 3s. net. 7½ x 5; 101; 1937.

The author relies chiefly on the work of Ogino and Knaus to fix the ovulation period between the twelfth to the sixteenth day, both inclusive, counting backwards in the menstrual cycle. After taking into account the probable duration of life of the sperm, and of the egg, tables are constructed indicating when intercourse must be avoided by women with regular menstrual cycles, the cycles ranging in the length from 21 to 44 days. Similar tables are included for women whose cycles are subject to irregularities. Blank charts for the recording of menstrual cycles have been added at the end. There is an index.

WOMAN'S PRIME OF LIFE. *Making the Most of Maturity.*

By Isabel E. Hutton. Emerson Books, Inc., New York. $2.00. 7½ x 5; 150; 1937.

This book treats the climacteric period of women in a sound way. Dr. Hutton has written especially for those women who have little general knowledge of the physiological processes of the female reproductive organs and who look with dread towards the trying period of the menopause. Much wise advice is given but the book is not intended to replace the guidance of the physician—rather to supplement it.

WUNDER DER FORTPFLANZUNG. *Eine Einführung in das Wesen des Lebens für Jedermann.*

By Curt Thesing. Gustav Kiepenheuer, Berlin. RM. 6.00. 9½ x 6½; 371; 1936.

This is one of the better popular introductions to the processes of propagation and heredity. It is an interesting and clear presentation, based on the most authoritative recent investigations. The bibliography is short, including only the more general German works. The illustrations are well chosen and elucidative and there is an index.

BIOMETRY

STATISTICAL TABLES. *Their Structure and Use.*

By Helen M. Walker and Walter N. Durost. Bureau of Publications, Teachers College, Columbia University, New York. $1.60. 9 x 5⅞; v + 76; 1936.

A FIRST COURSE IN STATISTICAL METHOD. *Second Edition.*

By G. Irving Gavett. McGraw-Hill Book Co., New York. $3.50. 9 x 6; ix + 400; 1937.

These two books are intended for beginning students of the elements of statistics. The Walker-Durost treatise tells the reader

how to put together tables, with a degree of detail that passes the bounds of pedantry and wanders off into the precious. Most of what is sound and useful in the book is commonplace—taught in virtually all elementary courses, and embodied in most text books. When the book goes beyond this range the judgment of most veteran statisticians will differ from that of the authors. To take but a single example by way of illustration: on p. 66 the authors set up a table (XVII A), criticise it, and substitute in its place as preferable two tables (XVII B and C). To any normal person, gifted with the habits of simple, clear-cut, logical thinking table form A is enormously superior to forms B and C. B and C *look* more impressive because they obscure the facts with mathematical symbols. Furthermore the cost of setting forms B and C in type is greater than that for form A (and the cost difference would be still greater if the unnecessary vertical rules with which form A is cluttered were removed). Also the chances of errors in composition (with subsequent proof correction) are much greater in forms B and C than in A.

Prof. Gavett's excellent elementary text is improved in the second edition. The most annoying feature of it is the absence of bibliography, suggested supplementary reading, or acknowledgment to earlier workers who originally ploughed the same fields. The author's habit of appropriating ideas from other authors and dressing them up just enough differently so that a change of plagiarism technically will not lie, falls something short of that generosity that formerly existed in the field of scholarship but now seems rather demoded in a generally smarter but less honest world.

MATHEMATICS FOR THE MILLION.
By Lancelot Hogben. Illustrated by J. F. Horrabin. W. W. Norton and Co., New York. $3.75. 8½ x 5¾; 647; 1937.

Those of our readers who are interested in the history of elementary mathematics, and how its various branches got "started" will find a great deal of valuable information in this book. The answer to the question "what can you do with mathematics?" constitutes the principle theme. One is shown how to obtain the height of cliffs and the width of rivers, how to find the area of triangles, how to construct a star map, and so on—all unquestionably very useful information. The author, between examples, which are numerous and well illustrated, drags in, for no apparent reason, just about everything from the theory of organic evolution to a character out of the latest H. G. Wells novel.

Now let us consider the reader who has some intellectual interest in mathematics, and is more concerned in what it is "about" than in locating the position of a star, or how to find the width of a stream. The chapter "What the Calculus is About" will serve as an illustration. Here the author demonstrates that the first derivative of a plane curve is the slope of the tangent line, shows the relation of the second derivative to the first in terms of acceleration, gives the more simple rules of differentiation, explains the integral as the limit of a summation process, and then adds some elementary integrations. This takes 56 pages. Any introductory text in calculus covers the same ground, and requires considerably less space to do so. The long winded "simplifications" involving trains that run between London and Edinburgh do not, in our estimation, add anything in the way of clarity.

In reference to infinite series the term "convergence" is standard usage, and "d^2x" is always called the "second differential." But in the interests of clarity Professor Hogben thinks it well to introduce "choking off," and "the second dwarf x."

Short tables of sines, tangents, logarithms, and squares have been included. Exercises appear at the end of each chapter. There is no index.

WIE STATISTISCHE ZAHLEN ENTSTEHEN. *Die entscheidenden methodischen Vorgänge.*
By Franz Zizek. Hans Buske, Leipzig. RM. 4.50. 9½ x 6½; iv + 151; 1937 (paper).

This is a rather general simple discussion of statistics for the student of statistics and also the layman who has to answer many questionnaires and often wonders what happens to his answers. The author considers his subject from three points of view for purposes of explanation. In the first place he deals with the individual phenomena or units that make up the statistical mass. In the second place he considers the purpose or goal for which the statistics are collected, and what conclusions can be drawn from the statistics. In the third place he discusses statistical methods. His methodology is limited to a practical discussion of the mean and standard deviation. No other constants are discussed, nor are any formulas given for derivation of these two constants. There is no bibliography or index.

BIOLOGIE MATHÉMATIQUE.

By V. A. Kostitzin. Preface by Vito Volterra. Armand Colin, Paris. 13 francs (paper); 15.50 francs (cloth). 6¾ x 4½; 223; 1937.

This is a compact exposition of the methods used in the mathematical analysis of biologic phenomena. The well known methods proposed by Volterra, Lotka, Pearl, and Haldane for the study of the growth of organisms and populations under different environmental conditions are synthesized and welded into one coherent system. Well selected examples are presented to illustrate the application of analytic procedures to concrete problems.

CONTINUOUS INVESTIGATIONS INTO THE MORTALITY OF ASSURED LIVES. *A 1924–29 Light and A 1924–29 Heavy. Mortality Functions and Monetary Tables.*

Published on behalf of The Institute of Actuaries and The Faculty of Actuaries in Scotland. Cambridge University Press, London. £2. 2s. 9¾ x 6; viii + 147; 1937.

Life tables, and auxiliary functions, of use to actuaries, based upon groups showing respectively low and high rates of mortality. The work has been done in the best traditions of the Institute of Actuaries in England. Earlier volumes in the series have been noted in these pages.

PSYCHOLOGY AND BEHAVIOR

PRIMITIVE BEHAVIOR. *An Introduction to the Social Sciences.*

By William I. Thomas. McGraw-Hill Book Co., New York. $5.00. 9¼ x 6; ix + 847; 1937.

This treatise presents a systematic survey of the patterns of human behavior as observed in primitive societies. The subject matter includes descriptions of characteristic attitudes and language forms, criterions of kinship and its related taboos, sexual behavior and ceremonies, forms of law and government. The three last chapters of the book are concerned with the author's ideas on culture patterns, illustrated by an outline of the development of Bantu culture, and with the perennial question of superior and inferior races.

In the exposition of the subject, the author ably illustrates his own viewpoint regarding the development of behavior patterns in human societies. These are that the theories which assume the existence of differences in mental and psychic characteristics among races or populations have not been proven. Instead, the author believes that diversities in behavior and culture patterns are due to different interpretations of experiences. Consequently, the theory of a uniform course of cultural and behavioral evolution cannot be accepted. Moreover, the author thinks that culture area rather than the physical environment should be emphasized.

In their adjustive strivings territorially isolated groups develop, through their specific experiences, characteristic values and habits, some of them unique, and the circulation of these traits, their migrations from area to area, and the borrowing back and forth, represents a sort of social inheritance, and is perhaps the main basis of social change and of advance to the cultural level termed "civilization"

One of the outstanding characteristics of this book is that the interpretations of ob-

servations always demonstrate a realistic perspective of social phenomena. The literature has been thoroughly reviewed and a good portion of the text consists of excerpts from the sources cited. The bibliography covers 36 pages.

LABYRINTH UND UMWEG. *Ein Kapitel aus der Tierpsychologie.*
By J. A. Bierens de Haan. E. J. Brill, Leiden. 4. guilders. 9¼ x 6¼; 231; 1937 (paper).

The aims of the author of this monograph are

(1) To introduce the reader by means of a historical and critical survey to the problems and the methods of maze and detour researches; (2) To give a summary of what has been attained with different animals in both fields, in which special attention has been given to the work done with the white rat in the maze; and (3) To try to combine the results of both the departments of research work and to find for them a common explanation, by conceiving them as a (primary or secondary) grasping of spatial relations with regard to a goal striven after by the animal.

The study opens with a discussion of Small's work and an elaborate description of different types of mazes. The detour work of Thorndike and later criticisms by Hobhouse are discussed. Then follows a survey of the work done on different animals including cats, dogs, non-domesticated carnivora, rats, the lower vertebrates, and invertebrates. Much of the work on the invertebrates seems dubious in the eyes of the author. The rest of the book is devoted to the learning of maze and detour by the white rat. The author sums this up and surveys it critically coming to the not very exciting conclusion that it will never be possible to formulate laws and rules that can predict accurately the behavior of any one rat in any particular maze. Only it can be said with certainty that finding the way in the maze is based primarily on a striving to reach a goal and is achieved through an acquired knowledge and idea of the apparatus and its structure and the path through it. The process seems to be similar to finding one's way about in a strange city. There is a detailed bibliography.

THE BIOLOGY OF HUMAN CONFLICT. *An Anatomy of Behavior, Individual and Social.*
By Trigant Burrow. The Macmillan Co., New York. $3.50. 8½ x 5½; xl + 435; 1937.

Neurosis, crime and insanity, the author believes, are the external signs of a universal "lack of coördination and peace that is primary and internal to man." This condition is perceptible within man when a non-mental (non-symbolic) method of observation is employed. It appears then that disorders of behavior arise from the conflict between "physiological tensions" due to the desire to maintain social security and integrity and those tensions caused by the necessity for social intercourse. In the realization of this fact lies the hope that human behavior disorders may be successfully treated and in the same manner as physical disorders. There is much to be said in favor of this theory which, however, is far from being completely original. But, a clear interpretation of the author's thoughts is not always possible due to his rather involved style and penchant for coining new words. It is also to be noted that in the preface and again in the concluding chapter the author states that this book is the outcome of an investigation of behavior problems conducted by and among the staff of the Lifwynn Foundation. In other places throughout the book this fact is mentioned, but nowhere is there a definite statement of the procedures employed in the investigations or of the actual results. Lacking these, the reader is unable to judge the significance of the author's conclusion.

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY. *Volume III.*
By James R. Angell, Frederick C. Bartlett, Madison Bentley, Harvey A. Carr, Sante De Sanctis, Joseph Fröbes, O. Klemm, Karl Marbe, Charles S. Myers, E. W. Scripture, Edward L. Thorndike, John B. Watson, Wilhelm Wirth. Edited by Carl Murchison. Clark University Press, Worcester, Mass; Oxford University Press, London. $5.00. 9 x 6; xvii + 327; 1936.

The purpose of this book is expressed in the introduction in the following manner:

> The author of a recent book of psychology found that it was impossible to get important facts concerning the scientific development of certain individuals except from those individuals themselves. Since a science separated from its history lacks direction and promises a future of uncertain importance, it is a matter of consequence to those who wish to understand psychology for those individuals who have greatly influenced contemporary psychology to put into print as much of their personal histories as bears on their professional careers.

The idea is a good one, and the volumes have a real and permanent value. But, as must surely have been evident to an editorial committee of psychologists different men do find widely varying degrees of difficulty in artistically stripping off their mental clothes, down to the nude, in a gold fish bowl placed in a show window on Main Street.

THE INTELLECTUAL FUNCTIONS OF THE FRONTAL LOBES. *A Study Based Upon Observation of a Man After Partial Bilateral Frontal Lobectomy.*

By Richard M. Brickner. The Macmillan Co., New York. $3.50. 7¾ x 5¼; xvi + 354; 1936.

In the course of two operations in August and September 1930, the patient, a man then 41 years of age, underwent a partial bilateral frontal lobectomy. Beginning in November 1931 efforts were made to re-educate him. Complete notes, and in many cases the patient's own words, were recorded by the various people participating in the re-education process. The author of the book arrives at no conclusions, but presents the material in such a way as to enable the reader to draw his own.

The book is divided into sections which include the patient's history prior to the operation, anatomical and surgical discussion of the operation, post-operative observations, symptoms in the intellectual sphere, psychological tests, and other similar useful information.

Since this is the only known case where the patient survived such an operation for several years and led "a more or less well adjusted life," this careful study should prove to be of great value to neurologists, psychiatrists, and psychologists. There is an extensive bibliography, an index of names, and an index of subjects.

A MIND MISLAID.

By Henry C. Brown. E. P. Dutton and Co., New York. $2.00. 8 x 5½; 219; 1937.

Any man who regains his normal faculties after a period of mental disturbance and then writes a good honest report of his experience does a big service to medical science. Every physician, and particularly every physician who deals with the psychopathic and the insane, should read such a report with the greatest care because it can teach him much.

Mr. Brown's breakdown came late in life, perhaps partly as a result of overwork, but largely because of a tremendous disappointment and psychic shock. The book might have been even more helpful if the writer had given us a better picture of his mental processes during the period of agitated depression, but there are many things in the book which will interest not only the psychiatrist, but also the layman, be he normal or psychopathic. One of the last chapters of the book will be particularly helpful to the relatives of the insane whose feelings are often harrowed by the fear that their loved one is being brutally handled. Brown has only kind words to say about the way in which he was treated during his long stay in the asylum. The book is interesting and reads easily.

A PRIMER FOR CRITICS.

By George Boas. The Johns Hopkins Press, Baltimore. $2.00. 8½ x 5½; viii + 153; 1937.

A thoroughly intelligent discussion of the difficulties encountered, and the distinctions that must be made in analyzing one's reactions to a work of art, whether the critic is the spectator, or the artist himself. There are chapters on instrumental values in art, terminal values, schools of criticism, and the authority of criticism. There are no elaborate discussions of

aesthetics, nor is there any attempt to furnish the reader with any sort of a scale for evaluating emotional experiences. The reader need not be deceived by the word *primer* in the title, for most persons who are interested in the arts could derive some profit from reading the book. As supplementary reading *Practical Criticism*, by I. A. Richards, would serve very well.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

ATOMS, MEN AND STARS. *A Survey of the Latest Developments of Physical Science and Their Relation to Life.*

By Rogers D. Rusk. Alfred A. Knopf, New York. $3.00. $8\frac{3}{4}$ x $5\frac{3}{4}$; xviii + 289 + ix; 1937.

Within the present generation there have occurred three fundamental changes in scientific conceptions. The first of these was the theory of atomic structure associated chiefly with the names of Rutherford and Bohr. The second was Einstein's theory of relativity, and the third was the quantum theory of Planck.

The ramifications of these theories are legion, and it is perfectly possible to be a useful and altogether admirable citizen without being cognizant of all the implications that depend upon them, but it is not possible to consider as liberally educated anyone who does not know a little about what they mean and why the earlier theories which they have displaced had to give ground.

For this reason a great multiplicity of books has appeared within the past few years, by the reading of which the man in the street is supposed to acquire a knowledge of what it is all about. The present work is one of the best, if not the best, attempt to achieve this result that has as yet appeared. It is not in any sense a popularization or a simplification, but is a straightforward effort to present in lucid language the latest developments in physical science.

Modern scientific theory is essentially esoteric, and he who would become acquainted with it must reply to the question "Understandest thou what thou readest?" in the words of the Ethiopian eunuch of Queen Candace, "How can I, except some one shall guide me?" Just how efficient a guide the present work is may be demonstrated by considering some of the instances in which it is likely to prove helpful.

Everyone today concedes the existence of subatomic particles, commonly referred to as electrons. But the reader of this work will be informed that the electron is only one of five different types of particles, and that the existence of two others has been predicated upon reasonable evidence. Also everyone is familiar with Mendeléjeff's theorem that the chemical properties of the elements are periodic functions of their atomic weights, but that there is a group of elements called the "home for incurables" which have fractional atomic weights and which will not fit into the scheme. It now appears that such substances are isotopic, i.e., their atoms do not all have the same weight. A fractional weight indicates a mixture of isotopes; a pure isotope always has an integral atomic weight and fits into the periodic table just as Mendeléjeff thought it should.

The interested reader will also find an explanation of why the constancy of the velocity of light as observed by Michelson and Morley makes inevitable the conclusion that bodies in motion are shorter in the direction of their motion than when at rest, and that their mass is greater by virtue of their motion.

The quantum theory is merely the atomic theory applied to energy. The atom of energy is the quantum. Just as the elements differ in the value of their atomic weights, so do they also differ in the value of the quantum of the energy derived from their oxidization. The quantum is inversely proportional to the wave length of the light with which it is associated, and Planck's constant "h" is the product of the value of the quantum by its associated wave length. Like the atom, the quantum can be divided into subatomic particles, but these while they resemble energy in some ways and matter in others, cannot be considered to be either energy or matter. One is tempted to paraphrase Augustine of Hippo and call them the primordial substance from which

the universe of energy and matter has been created.

Finally, the closing chapter is a masterpiece of expression. It is philosophical without being metaphysical, and thus is a worthy culmination of the whole work.

The author is not infallible. Especially is his geography a trifle shaky, as when he locates the two hundred inch telescope on Mt. Wilson instead of an Palomar Mountain, or Boulder Dam in the state of Colorado instead of on the river of the same name. But trifles of this sort do not unmake great books.

GARDEN RUBBISH AND OTHER COUNTRY BUMPS.

By W. C. Sellar and R. J. Yeatman. Illustrated by S. Dowling, Footnotes by Will Cuppy. Farrar and Rinehart, New York. $1.50. 7⅞ x 5¼; xvi + 146; 1937.

This is the type of book that seems destined to dissipate its saccharinity on the atmosphere of the uninhabited regions. In the first place it is a humorous book for gardners, a class in whom a sense of humor is conspicuously lacking, as is evidenced by the fact that they take themselves so seriously. Secondly, it is largely devoted to a discussion of beadles and cornucopias, both of which are somewhat mythical objects that have become practically extinct in the land of their origin, and are altogether unknown this side of the water. Finally, it is the output of English authors for American consumption, and between English and American humor there is fixed a great gulf.

But that is not half the trouble. The American can respect honest English humor even when he doesn't understand it. The present work exemplifies, not English humor but the English conception of American humor. As an example let us refer to the statement that Henry VIII's love for flowers was evidenced by the fact that one of his wives was named Anne Pollen. (This is not so bad as it sounds. The English really do pronounce Boleyn as if it rhymed with pollen). The book abounds in similar instances, which constitute a sad commentary on the effect of our humorous authors on the Englishmen who have read them.

In the entire work perhaps the cleverest bit of humor, according to western standards, is a quotation heading one of the chapters:

"A garden is a loathesome thing
So what?"

The brilliance of this parody causes it to stand out like the shadow of a mighty rock within a weary land, or a shelter in the time of storm, or a wart on the nose, or what have you?

This peculiarly oligocene type of humor is likely to appeal not so much to the floriculturist as to the folkloriculturist, and not so much to the agriculturist as to the gagriculturist. If there are no such words as these there ought to be, else how could we adequately express the American conception of the English conception of American humor?

A chief merit of this work is that it has no index whatever.

ADVENTURES IN ERROR.

By Vilhjalmur Stefansson. Robert M. McBride and Co., New York. $3.00. 8⅛ x 5¾; viii + 299; 1936.

The first two chapters are, with alterations from the author's book "The Standardization of Error," and the remainder is material from earlier magazine articles and public addresses. What differentiates Stefansson from practically all other debunkers is his expert knowledge (largely gained through personal experience) of the matters under discussion, and secondly, his notable talents for sardonic humor, exhibited at its best in Chapter 2, "The Pleasures of Buncombe."

The following is from the chapter on "Standardized Wolves:"

The Saturday Evening Post, Science Service, and the Department of Agriculture of the Dominion of Canada, powerful and worthy of all respect and confidence, are in this relation spokesmen too for a popular and scientific multitude. The wolf-pack, then, is secure even as things stand. But conditions are bound to trend steadily in their favor. As living beasts, wolves are getting fewer with the colonization of the wilderness, but wolf-pack stories do not thereby get fewer . . . witness how they come again

and again from districts where wolves no longer exist. The fewer the living wolves the less the chance of their being so studied that evidence against the pack habit can be gathered. Finally there will be no wolves left, except in zoos. The belief in packs will have survived the means of refuting it. It will have become a truth.

H. L. Mencken's now famous article on the history of the bathtub in America is reprinted in full. There is no index.

ELEMENTARY PHOTOGRAPHY *for Club and Home Use.*

By C. B. Neblette, Frederick W. Brehm and Everett L. Priest. The Macmillan Co., New York. 72 cents. 7¾ x 5½; viii + 253; 1937.

Self-directed activity is the keynote to the plan of this book. The subject matter is arranged into five units which form a series of project studies whereby the student becomes acquainted with the fundamental principles of photography through simple illustrative experiments, and is initiated into the art of picture taking through channels in which he learns by doing. The subject matter covered in the first four units includes the making of a pinhole camera, picture taking with hand cameras and the making of negatives and prints. Unit five furnishes pointers for the selection and use of different types of film and filters and points out various factors to be considered for successful purposeful photography. Abundant photographs are used to illustrate points brought out in the text. The book is designed primarily for schools or clubs (of the junior or senior high school stage) but is sufficiently complete to make it suitable also for individual self-instruction.

THE MARCH OF SCIENCE. *A First Quinquennial Review* 1931–1935.

By Various Authors. Pitman Publishing Corp., New York; Sir Isaac Pitman and Sons, London. $1.25. 8½ x 5½; viii + 215; 1937.

This report of the British Association for the Advancement of Science is the joint work of a dozen or so leading scientists of England to recapitulate the advances made in the various sciences during the past quinquennial period. The chapter on zoology is very comprehensive, but perhaps the best one is that on cosmic physics contributed by Sir James Jeans.

THE PRICES OF BIOLOGICAL BOOKS IN 1937

By RAYMOND PEARL and MAUD DEWITT PEARL

Department of Biology, School of Hygiene and Public Health, Johns Hopkins University

WHEN the QUARTERLY REVIEW OF BIOLOGY began publication in 1926 the custom was inaugurated of reporting at the end of each volume on the cost of the books that had been reviewed in its columns during the year. The present paper, therefore, is the twelfth of these reports on the cost of biological books. The prices of foreign books have been converted into dollars on the basis of the exchange at the time the books were received. Table 1 shows the findings for 1937, arranged in the customary manner.

TABLE 1

Prices of biological books, 1937

ORIGIN	TOTAL PAGES	TOTAL COST	PRICE PER PAGE
			cents
Germany	13,374	$260.91	1.95
British American	6,223	89.32	1.44
Great Britain	15,927	202.54	1.27
United States	86,398	805.93	0.93
Other Countries	3,588	30.68	0.86
France	9,087	77.09	0.85
British Government	1,277	4.32	0.34
U. S. Government	4,486	6.95	0.16
Totals and weighted average, 1937	140,360	1,477.74	1.053
Totals and weighted average, 1926–1936 incl.	1335,258	14,605.87	1.094

The total number of pages reviewed in 1937 is 140,360, an insignificant increase over 1936 but an increase of approximately 70 per cent over 1926, the year in which these tabulations began. In the twelve years of the QUARTERLY REVIEW's history the books reviewed in these columns have aggregated a total of 1,475,618 pages. To American buyers these cost in the aggregate a total of $16,083.61, leading to an average price per page for the total of 1.090 cents. The weighted average cost per page of 1.053 cents for all the books reviewed in 1937 is 3.7 per cent lower than that for all the books reviewed in our columns during the preceding eleven years 1926–36 inclusive, taken as a bulk total. It is lower than the corresponding average for 1936 of 1.147 cents per page by 8.2 per cent. The 1937 average price per page for all books reviewed is 4.0 per cent lower than the corresponding figure for 1926, which was 1.097 cents. The general picture presented by the 1937 summary is clearly of lowered prices. The American biologist buying books in 1937 got off relatively easily, as compared with other years, as judged by our review sample.

In 1937 Germany returned to her customary position at the head of Table 1, as the source of origin of highest prices for biological books. And, in general, all the sources of origin stood this year in about their usual order. It does not appear that the announced policy of the German publishers relative to book prices that has been discussed in these columns during the past two years has produced much of a realistic effect in lowering average prices, as yet.

Following the custom inaugurated last year Table 2 shows the price trends of books published in various countries during the decade from 1928 to 1937 and the absolute and relative changes in price from 1936 to 1937 and from 1928 to 1937.

The average prices per page of our samples of biological books from every origin except Germany and Great Britain were *lower* this year than in 1936, by amounts ranging from about 10 per cent for books commercially produced in the United States to 79 per cent for the British Government official publications. The 1937 average price per page for books biologists, by 6.0 per cent over 1936, and by 31.8 per cent in the last decade.

Table 3 sums up the whole twelve years experience of the QUARTERLY REVIEW.

It is evident, from what has now grown to be a substantial sample, that during the past eleven years biological books from all over the world taken together have averaged to cost the American biologist very close to a cent a page, taking good, bad, and indifferent together. Furthermore it is plain that the sources of origin of these books fall into three fairly sharply defined groups relative to unit prices to the American buyer. In the first or rela-

TABLE 2

Comparison of the prices of biological books for the decade from 1928 to 1937

ORIGIN	AVERAGE PRICE PER PAGE										CHANGE + OR − FROM 1936 TO 1937		CHANGE + OR − FROM 1928 TO 1937	
	1928	1929	1930	1931	1932	1933	1934	1935	1936	1937	Absolute	Relative	Absolute	Relative
	cents	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*per cent*	*cents*	*per cent*
British-American	1.46	1.90	1.91	2.27	1.48	1.29	1.45	1.53	1.81	1.44	−0.37	−20.4	−0.02	−1.3
Other countries	1.13*	1.68	0.97	1.53	1.02	0.85	0.86	1.20	2.26	0.86	−1.40	−61.9	−0.27	−23.9
Great Britain	1.09	1.29	1.13	1.19	0.89	0.66	0.96	0.84	0.94	1.27	+0.33	+35.1	+0.18	+16.5
United States	1.14	1.14	1.09	1.05	1.00	1.02	0.93	0.90	1.03	0.93	−0.10	−9.7	−0.21	−18.4
Germany	1.48	1.65	1.82	1.75	1.60	1.43	1.89	2.04	1.84	1.95	+0.11	+6.0	+0.47	+31.8
British Government	1.26	0.39	1.19	1.03	1.45	1.39	0.89	0.50	1.62	0.34	−1.28	−79.0	−0.92	−73.0
France	0.45	0.47	0.47	0.69	0.60	0.74	1.00	0.86	1.05	0.85	−0.20	−19.0	+0.40	+88.8
U. S. Government	0.21	0.23	0.30	0.28	0.36	0.17	0.18	0.11	0.21	0.16	−0.05	−23.8	−0.05	−23.8

* With two special treatises omitted as explained in Vol. 3, p. 601.

commercially published in the United States was 18.4 per cent lower than that shown by our 1927 sample. The rise in the per page price of biological books published in France, which has been commented on in these notes in recent years, continued in 1937 and amounted to 19.0 per cent over 1936 prices and 88.8 per cent over those of a decade earlier. Our sample of biological books from British commercial publishers showed a 35.1 per cent advance in price over the 1936 sample. German books, as judged by our samples, increased in price in 1937 to American

TABLE 3

Average biological book prices over the twelve year period, 1926–37 inclusive

ORIGIN	TOTAL PAGES	AVERAGE PRICE PER PAGE
		cents
British-American	102,415	1.606
Germany	164,943	1.587
Other Countries	49,761	1.327
United States	863,508	1.030
Great Britain	119,861	1.026
British Government	8,836	0.931
France	122,222	0.694
U. S. Government	44,072	0.235
Total and weighted average	1,475,618	1.090

tively high priced group fall books in the British-American, Germany, and "Other countries" categories of origin. The next or medium priced group includes the United States, British Government, and Great Britain (commercial publishers). The average per page cost of biological books has been very nearly the same for these origins. Finally the third or relatively low price group includes books published in France and by the U. S. Government.

The reader should bear in mind that these reports are based on small samples of books in general and, for some countries, on small samples of the biological books published. He should therefore be cautious in applying conclusions drawn from this material to the general domain of book prices.

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